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ECLECTIC TREATISE
ON THE
PRACTICE OF MEDICINE,
EMBRACING
THE PATHOLOGY OF INFLAMMATION AND FEVER,
WITH ITS
CLASSIFICATION AND TREATMENT.

BY
ROBERT S. NEWTON, M.D.,

PROFESSOR OF CLINIC MEDICINE AND SURGERY IN THE ECLECTIC MEDICAL INSTITUTE
OF CINCINNATI, OHIO.

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TO THE READER.

In conclusion, it is proper to remark, that when we began this work, it was our intention to make a complete work on Practice. Having concluded to go no farther for the present, this portion is submitted to the reader, believing that the true pathology has been laid down, and if made the basis of practice, it will, in every case, demonstrate fully the truth of the doctrine taught in the preceding pages.

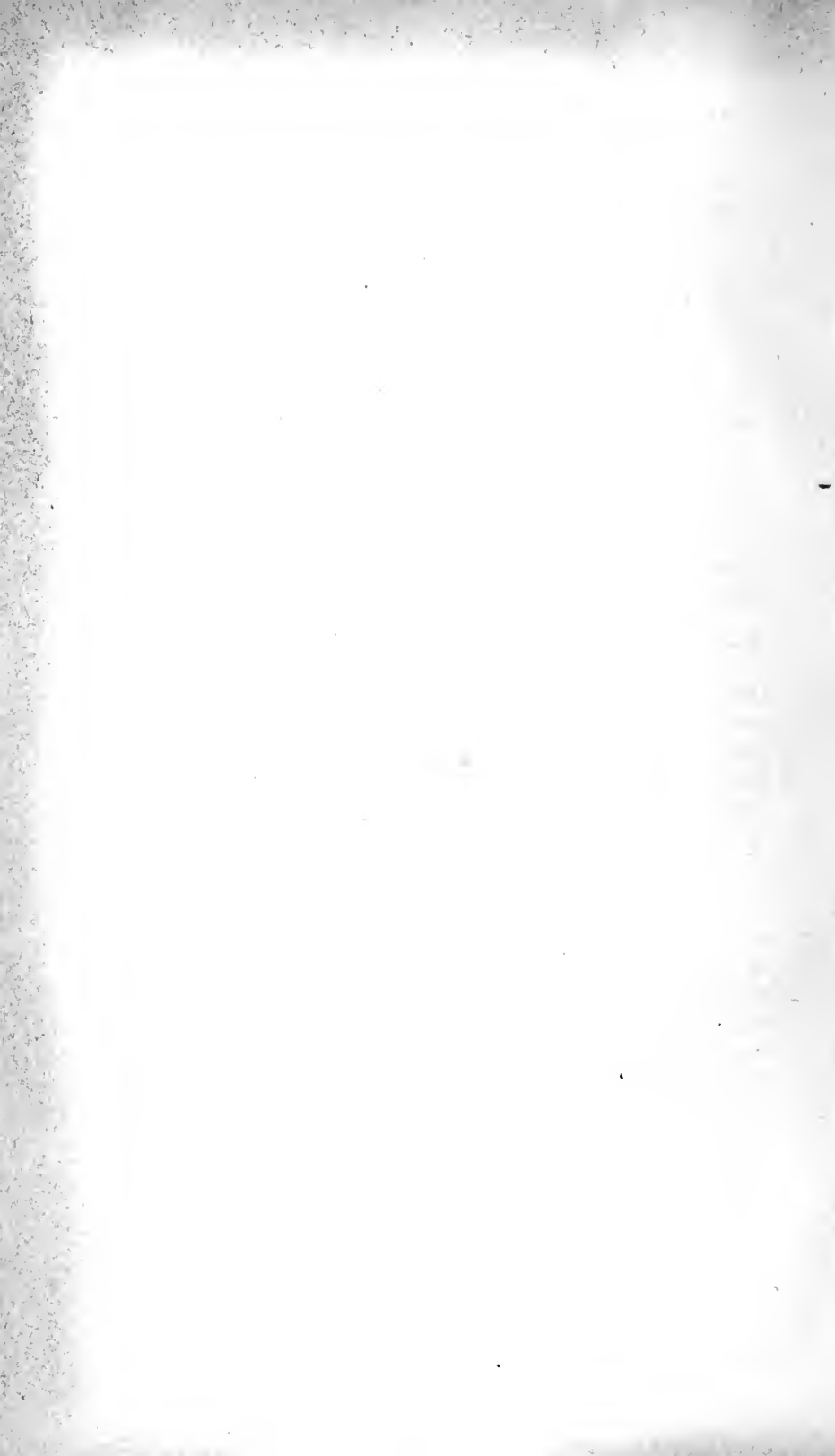
Twenty years' practice has given us a fair opportunity to judge of the various plans of medication based upon the several theories of pathology, and after trying, fully and fairly, the Allopathic and Eclectic modes and results, we are forced to say, that at least fifty per cent. of cases are in favor of the latter.

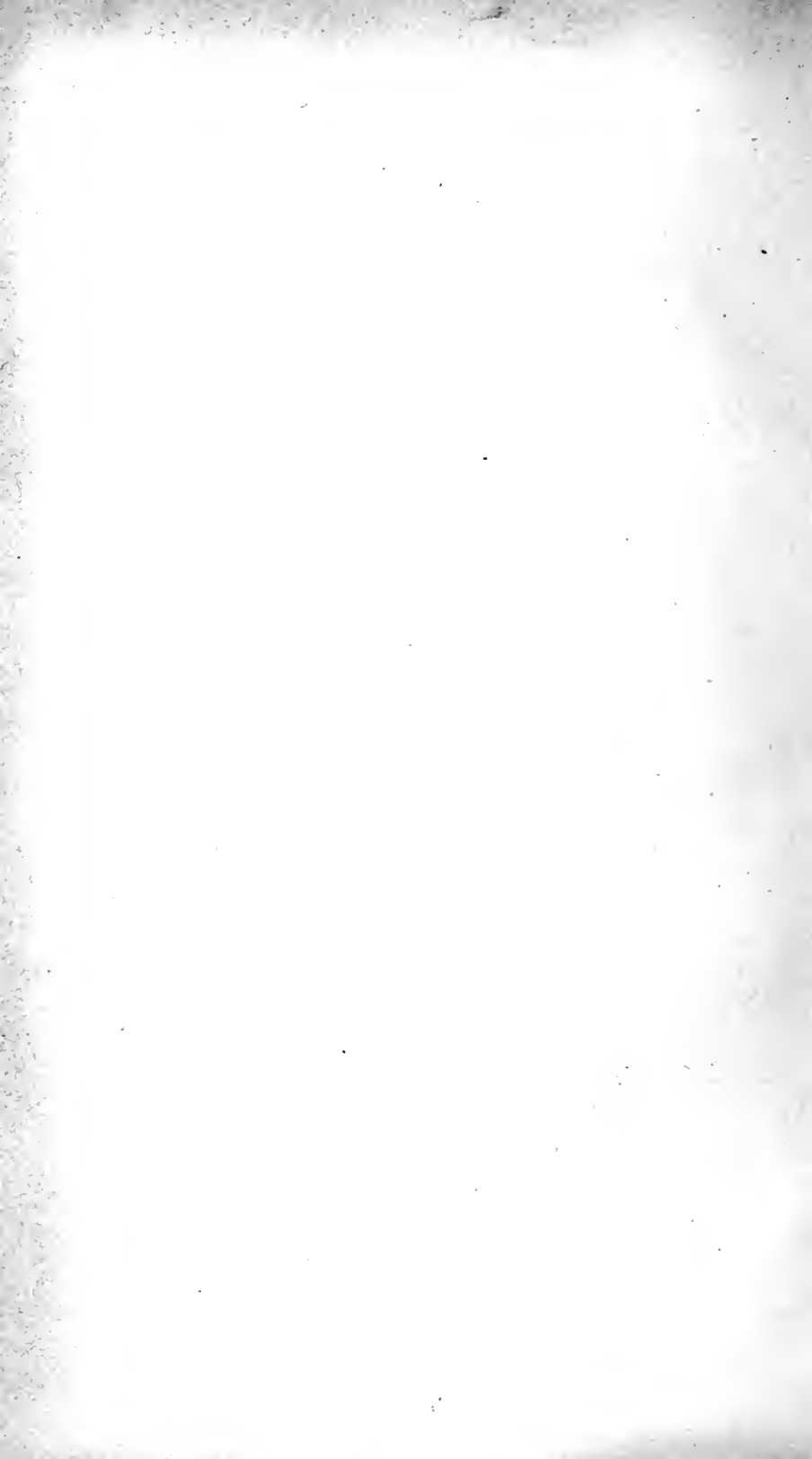
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AN ECLECTIC TREATISE

ON THE

PRACTICE OF MEDICINE,

CHAPTER I.

THE PATHOLOGY OF INFLAMMATION AND FEVER—FEVER AND INFLAMMATION—A PATHOLOGICAL CONDITION UNDER CIRCUMSTANCES—THE DISCUSSION OF DRs. BENNET AND ALISON, OF EDINBURGH, UPON THE CHANGE OF TYPES OF DISEASE—IS THE TYPE OF DISEASE THE SAME AS IT WAS TWENTY-FIVE YEARS AGO?

THERE are two states of the human body—a state of health and a state of disease. The former is maintained by a due equilibrium between the various normal functions of the human organism; and the study of that equilibrium constitutes the science of physiology. The latter is the result of a departure from the physiological equilibrium, and the study of those departures constitutes the science of pathology. The former study is essentially necessary to enable the practitioner to perceive and comprehend those pathological states whose manifestations constitute disease. There are some general physiological laws, which it is believed every medical man either does or ought to comprehend; or, when he undertook the care of a case, he would be very much in the condition of a blind optician, or a deaf newspaper reporter. So, too, there are some general principles of pathology, which ought to be fully comprehended by every practitioner.

I have set out to write an eclectic treatise on the practice of medicine, and this I mean to perform with reference to the requirements of those who are actually engaged in the exercises

of the healing art. I shall endeavor to produce a series of pages worthy of confidence, and one which shall contain such information, and such only, as will be of service to the practitioner. There is no such book in existence; those that we have being too exclusively theoretical, or otherwise greatly deficient. During the past sixteen years, I have been in the daily practice of medicine and surgery, and for the last ten as a lecturer on the practice of medicine and the principles and practice of surgery, and my own experience of what kind of a work is needed will direct me in the preparation of this.

The science of medicine, though of great antiquity, is still very imperfect. When physiology, pathology, anatomy, etc., had hardly yet begun to claim special attention, of course the practice was little better than mere guess-work; but as physicians came to the study of the special departments, as truth after truth was unfolded, so the old errors were cast off and new principles were adopted. Works written by the most eminent physicians a century ago, are now considered exceedingly imperfect, so much so, that even within the last twenty-five years, the science of medicine may be said to have been almost entirely revolutionized. This change of practice will continue until it shall have been based on a clear understanding of the great laws of health and disease.

It is my purpose to represent the theory and practice of the most eminent physicians of the present day—1860—and in a mode which all can appreciate. Against the errors which have been heretofore practiced, I shall be compelled to maintain a steady opposition, and shall thus, no doubt, fail to meet the approbation of the entire profession. This I expect—for those who have been educated to the old practice, will not readily admit that they had really not understood the science of the healing art. When we see the leading medical men of the world reversing their practice and suppressing their earlier works, we may well inquire if it is not high time that their disciples and pupils were beginning to question their own views.

It will be understood that there are principles in medical science which are true, and these are the same now as in the palmy days of Hippocrates—for truth is the same in all time and in all countries; and it is the search after these principles which has engaged the earnest attention of so many eminent men for so many ages. These general principles, as far as understood, will be left untouched, except as they can be used to show the bearings of those principles which have been more recently discovered.

It is, perhaps, known that the idea of eclecticism has been strongly opposed by the great mass of American physicians;

yet, within a few years, the very authorities whom physicians have relied on have abandoned the old positions, and, to a great extent, have admitted the very principles for which the eclectic branch of the medical profession has been contending. At this moment the Medical Faculty of the University of Edinburgh, are greatly divided upon the propriety of blood-letting in the treatment of pneumonia, which has shown at least one fact which ought to claim our attention, viz: That the soundness of the theories upon which the commonly received text-books on practical medicine have been based, are called in question by some of the most eminent medical men in Europe. The question seems to have been sprung by Dr. Bennet, the Professor of Clinical Medicine in the University of Edinburgh, and Drs. Alison, Gairdner and others have been forced to take part. Let it be understood that I have long opposed the use of the lancet in the treatment of pneumonia as well as in other diseases, as I have the indiscriminate employment of mercury, lead, arsenic, etc., and that others had opposed them long before I came into the profession, and before Dr. Bennet had ever written a medical essay; and it will be apparent that for the new pathology he is not responsible, the principles and old theories having been long before his time exposed by Beach, Morrow, Jones and other practitioners in the United States—as early as 1830, over a quarter of a century ago. It is not for me to say why they opposed the doctrine of Broussais and others of his school, but, from their publications, it seems to me that they were governed more by experience than by a clear comprehension of the pathological errors then in vogue.

Since 1830, many facts and principles in physiology have tended to a more perfect understanding of that science, the result of which has been a clearer conception of those departures from healthy action which are denominated pathological. In the outset of this work it may be inquired how and in what way we are to be guided by the opinions of those who have written on medicine. The theories that have been advanced from age to age are almost valueless, but the facts that have been adduced, the experience that has been recorded, should weigh very heavily with those engaged in teaching modern medicine; unless, indeed, we accept the position of Dr. Alison, who broadly affirms that the types of disease, and, therefore, the human constitution, have undergone marked changes within the last thirty years. If such be a fact, the experience of those who practiced thirty years ago is only good so far as their own times were concerned. If this proposition of Dr. Alison be well founded, then it is perfectly apparent that medical science can never be otherwise than empirical, an

inference which I am not willing to admit. But that Dr. Alison has been able to find many of his old pupils to testify to the correctness of his position—the change of the type of inflammatory disease is, to me, no wonder—and as the settlement of this question lies at the very basis of the whole practice of medicine, I propose, without reference to the arguments of Dr. Bennet, to show that Dr. Alison has been driven into an extraordinary mode of bracing up his position, especially on a question of such scientific importance.

Uninterested in the personal merits of this dispute, I am compelled to lay aside every prejudice, and mediate, to my own satisfaction, at least, between Dr. Bennet on the one hand, and Dr. Alison and his disciples on the other. Had Dr. Bennet modified a few expressions, his theory would have been unobjectionable. The facts, as detailed by him, are essentially correct, but I must be permitted to think that he has not done all that was in his power for the elucidation of his own theory, while, also, he has hardly taken Dr. Alison's proposition for what it is worth. So, too, Dr. Alison stands in his own light by the bungling way in which he has arrived at his prominent conclusion—from insufficient data. If Dr. Alison had broadly asserted that the *phases of disease* had been modified within the past thirty years, then it seems to me that his position would have been defensible. Dr. Alison does not stand alone in his position any more than does Dr. Bennet. Both are warmly supported, but, unfortunately, not in the way which could strengthen either. Dr. Watson, Dr. Symonds, Sir Chas. Hastings, Sir John Richardson, Dr. Shann, Mr. Robertson, Dr. Harvey, Dr. Fry, Dr. McCormac and Mr. McFarlan, in their several letters to Dr. W. P. Alison, in the *Edinburgh Medical Journal*, only reiterate the statement of Dr. Alison, that the *type* of inflammatory disease has changed from what it formerly was; and, strange to say, not one of these eminent men has attempted to give us the *rationale* of that change. They have contented themselves by simply asserting that patients laboring under inflammatory disease, do not bear copious blood-letting as was formerly practiced, and that therefore the type of inflammatory disease has changed. This is coming to a conclusion from experience it is true, but experience of a limited and very imperfect nature. Both parties in this dispute have truth to sustain them, both are governed by experience, and it remains for some one who is a disinterested observer to bring together and harmonize the statements respectively of Drs. Bennet and Alison. I am free to admit the difficulties which present themselves are enough to deter me from the hazardous attempt; nevertheless, if I am to produce a book worthy of professional confidence, I must boldly meet this

issue, for upon the settlement and adoption of one or the other of these questions, or of both modified and subjected to the rigid tests of reason and experience, will depend the soundness of the practice which I am about to present to the medical public. If the question is so fraught with perplexities and sources of error as to deter such men as Alison, Watson, Gairdner, McFarlan, Hastings, and others who believe with Dr. Alison, I may well be pardoned if I fall into a few errors as to the bearing of facts. I begin by committing myself to the following propositions, which I shall endeavor to elucidate afterward:

First. The type of inflammatory disease has not changed.

Second. The phases of inflammatory diseases have changed.

Third. Inflammation has not heretofore been well understood.

Fourth. The system does not possess the stamina now that it formerly did, and does not, therefore, bear depletion by blood-letting as it formerly did, but that this depends on modified surrounding influences, and not upon any actual change in the type of disease.

As the dogmatic assertion of my first proposition is not to be taken upon my mere *ipse dixit*, I shall be expected to establish it by argument, by fact, and by inference. I therefore enter upon the task, as I firmly believe, in the rejection of opinions merely which do not bear investigation.

Man is essentially a creature of conditions, and liable to the impression of many influences. Air, water, and combustible matter constitute three chemical conditions, neither of which he can dispense with. Let us imagine a mature man, in the vigor of health and life—his health presupposes the supply of air, water and oxydizable matter. There is not only a supply of all these conditions, but there is a true relation of equilibrium between them. If either one be denied him his body is destroyed. If the air be cut off he is no longer able to carry on interstitial oxydation—his blood ceases to circulate—he dies, in a word. If the water is denied him, the processes of oxydation, which are productive of heat, being uncontrolled, he is consumed by fever; and if combustible matter be denied him, his own tissues are oxydized. If air, then, is so very essential, we are not troubled to perceive how he is affected when it is changed in quality or quantity. His life depends on the interstitial oxydation that is going on within his body, and this oxydation depends on the oxygen which he derives from the atmosphere. If, therefore, he is compelled to breath an atmosphere poor in oxygen, one of two things must occur: we must furnish him with material for digestion which is more readily oxydized, or else a lethargy of combustion and functional action

occurs, which unfits the system for excreting and secreting as in ordinary, when we get an ænemic state, and presently there is obstruction of some organ or apparatus, and we get inflammation or fever as the result. So, too, if allowed to breathe air rich in oxygen, and he also be furnished with a plentiful supply of combustible matter, the equilibrium of health will be destroyed, and fever of a high grade sets in. Or if furnished with a plentiful supply of air and water, but he is denied combustible matter, his own tissues are consumed, and he dies from a low grade of fever in the end. Let us bear these facts in mind, for they will come upon us forcibly when we come to inquire into the nature of the changes which inflammatory diseases have really undergone. That oxygen does exercise a most important influence in the economy, would be inferred from the fact that a man will consume over eight hundred pounds per annum. These eight hundred pounds of oxygen are necessary to oxydize eight hundred pounds of solid food taken in during a year, and to control this combustion, there is also introduced fifteen hundred pounds of liquid—water, etc.,—in the same time, thus showing the introduction of over a ton and a half of matter into the body every year, which alone ought to be sufficient to point out the immense changes constantly going on in the body. Every motion, every thought is at the expense of the organic tissues, and hence, this immense supply of material to repair such wastes. Truly, in the language of Dr. Jno. A. Draper, “the condition of life is death.”

There is also thrown out or excreted during the same time an equal amount of matter, to-wit: over three thousand pounds, or more than twenty times the weight of his own body. For the excretion of this large amount of matter, we have certain apparatus, as the skin, kidneys, lungs, salivary glands, etc., which normally excrete matter only after it has been oxydized in the system, and whatever appears in the excretions is commonly in the form of oxydes; unless, perhaps, we except the milk or the secretions of the mammary glands. Very much of the fecal matter has never really been in the body in consequence of its being non-combustible at a temperature of 100 deg. Fahrenheit.

Everything in nature is in a state of mutation—ceaseless change is stamped on everything; but the laws by which those changes are produced are as immutable as truth itself; and whenever we are told that types have changed, we are in effect told that laws have changed, which is simply an impossibility—a scientific absurdity. Physiology tells us that no article is fit for food that will not, at a proper degree of temperature, and a plentiful supply of oxygen, take fire and burn, giving out, as the products of such combustion, carbonic acid gas and

water, or these with nitrogen or its compounds. If such be the fact, and no one questions the statement, then variations in diet will produce changes in nutrition, and therefore in organic actions. Thus the Irish people have changed their diet within the last three centuries, and the phases of disease depending on suspended or perverted nutrition, have been correspondingly modified. So in England in relation to the use of tea, and in the United States in regard to coffee. The use of tobacco has induced vicarious action in the glands of the throat and mouth, and to that extent have the phases of glandular disease been made to assume new phases. Were these inferences not clearly deducible from the facts, why do we order particular diets in certain diseased conditions? We all know that for the vitality of organization there are required many saline substances, which are to be introduced in a state of solution. This introduction is accomplished by water, and if the system shall be deprived of a plentiful supply of water so that these salts cannot be dissolved and deposited, or if this water shall be either deprived of a supply of saline ingredients or too thoroughly impregnated, then the general economy will be equally unfitted for the manifestation of healthy action. The phenomena of the circulation depend on oxydation, and if the air introduced into the lungs be poor in oxygen, combustion will be retarded, and every vital function will be accordingly modified. This has been done to some extent, *i. e.*, the air has been impoverished by the clearing of large areas of country, which has not only left a surplus of carbonic acid gas in the atmosphere, but it has likewise increased the extremes of heat and cold and also the process of evaporation, and just to that extent has been modified the phases of all those diseases which are strictly of a febrile or dermoid character. Everybody knows the influence exerted over evaporation by different states of the atmosphere.

If we live on a diet that is non-productive of the elements of casein, we can not expect that the milk will be rich in that element. Again, when a man is fed on a vegetable diet, there will be found in his blood much less fibrin than if he had been fed on animal food; yet the blood of carnivorous animals contains less than that of herbivorous animals, while birds have the most of all. But we are immediately concerned only so far as man is interested. The people of the United States eat less flesh now than they did thirty years ago, and greatly less than they did sixty years since; hence, it was more abundant in the blood during acute inflammations sixty years ago than now. Venesection was formerly resorted to in order to check its development, but, as is now generally admitted, in vain.

The plastic power, or principle of life, has not changed, though whole series of animals and vegetables have become extinct. Coal deposits reveal to us the significant fact that there has been withdrawn from the air an immense quantity of carbon, and that this, to some extent, may account for the extinction of races of animals. Again, after the surface temperature of the secondary period had fallen sufficiently low, then the animal kingdom was distributed in relation to the influence of the sun's rays. At the time the carboniferous deposit occurred, as a chemical necessity, there was an immense evolution of an equal volume of oxygen gas, and the hot-blooded animals immediately came forth as the result of favorable conditions. It is pretty clearly established that the human race has sprung from a standard type, and that all the departures from that standard man are results of physical influences, with the nature of which every physiologist is more or less acquainted. Draper and Pritchard think that ideal type may still be recognized. Nott, Glidden, Agassiz and others, contend that there are many types, but I see no proof of this; and admitting it to be true, it would not assist Dr. Alison's position, since any number of types less than the individuals of which the race is composed would still be modified as individuals are compared with the standard; and if one has departed from the true type, such departure must have a definite cause, which would operate alike on all of his tribe, and thus present us at last with a whole tribe that had departed from its type. A phase differs from a type. A type is the ideal perfection of an object—one that is regarded as embodying all the elements of perfection, and hence, Adam is regarded as the true typical man, and every phase of humanity is only a modification of that type. A phase is the particular aspect which a type has assumed under modifying influences. The golden pippin is a phase of a type represented by the common crab, and if the influences of art and cultivation be removed the pippin returns again to its type—the crab. The phases would change but not the type.

The osseous system of man is shown by those derived from the Eastern hecatombs, as mummies, some of which are at least three thousand years old, not to be different from what it is now. We find the same number of bones as at present, and for them much the same composition, except in those perishable products which have been dissipated in the process of decay. In a word, the anatomy of man was, a thousand years ago, as it is to-day, and if his muscles are less developed it follows as a result of non-exercise, etc. In this way man has been anatomically modified, but his type is the same. Every vital process has always been executed as at present, the cells,

of which the tissues are composed, are unchanged, and because they are not so violently involved in disease as formerly, we are not justified in saying that therefore the pathological types are changed. Place a body amid influences similar to those which surrounded men three thousand years ago, and diseases attacking him will not only have the original type, but they will have the same phases. Again, if the types of disease thus change, as is asserted by Drs. Alison, Watson and others, we know not how soon some new and universally fatal epidemic may sweep the whole race from the face of the earth. If it be true, what Dr. Watson has written in his time will, in all probability, be a tissue of absurdities in the times of his great grandchildren; which, for the honor and dignity of medical science, I am not willing to admit.

If changes appreciable to our senses have taken place in regard to the type of disease in the short space of thirty years, what will be the features of all inflammatory disease one thousand years hence? The changes observed are only modifications of manifestation—the type is the same. This idea may be clearly illustrated by another application. In the case of insanity, is the mind, the intellectual principle, changed, or is the manifestation merely altered? Evidently, the latter thing occurs—the intellectual principle is the same in both cases. As in the case of a flute in the hands of one who does not understand using it, the instrument gives out discordant sounds, yet those same sounds, when the instrument is rightly handled, are reduced to great harmony. The sound, the musical element is the same in both instances, but the manifestations are different. The type remains unchanged, while the phase greatly alters. So with respect to the type of inflammatory disease.

I have asserted that the nature of inflammation has not been heretofore well understood, and the various theories in relation thereto prove my position most conclusively. The quarrel between the parties already named would never have arisen had this been so. The great error of most writers on the subject, has been in treating it as essentially a disease *per se*, instead of regarding it as symptomatic of pathological states. Now, I hold that, strictly speaking, no inflammation can arise that has not been preceded by a cause—that the inflammation is a result of a cause, and in its treatment we should medicate with two objects in view, 1. To remove the cause. 2. To stay its progress until such obstructions or causes may have been removed. The various medical sects have arisen in consequence of the dissimilar views entertained on this very question, and while almost every theory is supported by more or less truth, each likewise contains much that is erroneous,

and from a wish to employ the truth only, I have been compelled to examine and re-examine most of these theories.

The stamina of the physical constitution is not now so strong, generally, in this country as it formerly was, and this circumstance is explained by an investigation of the influences bearing on society a century ago, as compared with those influences which impress us in 1860. The public does not now so generally resort to those more athletic exercises—hunting, running, jumping, lifting, etc.,—that were common sources of amusement one century ago. The public is less exposed to the vicissitudes of weather than formerly, the diet is more refined and varied, the clothing is better, often thinner however; carriages have taken away the practice of horseback exercise to a great extent; railroads have induced us to take frequent trips from home, so that we no longer see children engaged in sports which were common in our infancy. Our manners and customs have undergone a serious revolution, and every change impresses its influence on our constitutional stamina.

It is a law well understood by physiologists, that where the waste is greatest, there the greatest proportionate quantity of blood will be sent. If one of the limbs, as an arm, be powerfully exercised, the waste will be very great, and there will be sent an increased flow of blood to the arm at the expense of all other parts of the body. So if the brain and nervous system are almost exclusively exercised, the waste will be proportionally repaired by an increased flow of blood to the brain, to the depreciation of the more physical part of the body. The muscles decrease, the various functions of secretion and excretion are retarded, and of course the entire functional equilibrium of the man is more or less unbalanced. Do we not know that the brain and nervous system have been very greatly exercised during the past sixty years, and that this is being daily increased? and just in that proportion is the constitutional stamina weakened, and consequently the phases of disease have been modified, as is recognized by every intelligent physician who will take the trouble to look for himself. Hence, while blood-letting may have been practiced with some ingenuity in years gone by, it is certainly an exceedingly injurious practice under the weakened constitutional stamina of the present day. And irrespective of the arguments adduced by Drs. Bennet and Alison, the reader will understand why this is the case, and at the same time escape the error of Dr. Alison, that there has been a change in the type of inflammatory disease, or that of Dr. Bennet, that there has been no change, but a more definite and perfect understanding of human pathology.

Having said this much, Dr. Gairdner, who has a paper in the *Edinburgh Medical Journal* for September, 1857, may be

taken to task, for if his positions are right, then Dr. Bennet is wrong, and as it is the truth that I am seeking to elicit, a short paragraph or two will not be amiss at this place. Dr. Gairdner's paper sets out with the declaration that "blood letting has been the subject of a never-ending discussion from the earliest ages of medicine till now." If this declaration is true, we are enabled to come readily to, at least, one conclusion, which will palpably present itself to all sane minds, that it has been, at all times, seriously questioned, and that even in the palmiest days of the practice, there were men who would and did denounce it as a pathological error—that the practice was never, at any time, universally admitted by all physicians, which is presumptive evidence that the nature of the results of venesection were never thoroughly understood. In earlier ages, the practice, as is admitted by Gairdner, was "extravagantly lauded"—they claimed more for it than it deserved—and this is now known to have been the case with Gregory, Broussais, Bouillaud, Rush and others, of a later date. Venesection produced marked and visible changes which were at once recognized by the person who had submitted to the process, and for this, if for no other reason, it became somewhat popular. If one labored under a pleurisy, a copious blood-letting gave immediate relief, and the patient and physician gave attention to the immediate, rather than the remote results.

Upon Dr. Gairdner's authority I am justified, however, in making the statement that the opposition which has at all times been urged against the use of blood-letting, has been made by "distinguished leaders in medicine," and we may therefore reasonably conclude that the opposition was based on sound reasons. Nor were these opponents few in number, for Dr. Gairdner tells us that blood-letting has been time and again consigned to oblivion by "whole generations of men," but that it "rose again into an estimation more considerable in proportion to the previous neglect." This latter statement shows conclusively that the history of venesection has been very much as the history of many other errors: when men of moral courage rise and put them down, it is at most only a triumph for their own times, since others, whose only aim is money, soon set them on foot again to serve their selfish or ambitious ends. Dr. Gairdner thinks that the "opinions of any one man, or any set of men," can go only a little way in deciding the question of blood-letting. And if their conclusions are founded on "opinions" merely, I fully agree with him; but one person, who has carefully investigated a subject, may have collected facts enough to decide a given question with great certainty.

Medical men in all ages have resorted to experimentation,

and especially so in later years, which has been recorded in our literature. Now, if we endeavor to bring the facts thus demonstrated to bear on a particular point, the one who may undertake such a work, will, in all probability, reverse medical opinion on that particular point to a considerable extent. This has been often demonstrated in the progress of medical science, as may be learned by the labors of Harvey, Bell, Hall, Carpenter, Davy, Draper and others. In another sense, by Hippocrates, Cullen, Broussais, Beach and others. Upon the doctrine for which I contend—the immutability of the types of disease—these experiments—the collected facts brought to light by the labors of the illustrious dead—are guide posts to direct us in our scientific researches. If not so, why do we encumber our shelves with the works of Hippocrates, Thessalus, Chrysippus, Erasistratus, Van Helmont, Galen, etc., etc.? If recorded experience is valueless, then why do we so often quote the experience of those who have gone before us? The fact is, no matter what Dr. Gairdner or Dr. Bennet may think, every physician relies very much upon the observation and experience of those who have practiced medicine before he was born; and so universally is this principle admitted, that even the community always express great reliance in men who have had much experience in the observation of disease. Now, it is plain that if the type of disease has so changed within thirty years as to make blood-letting, which was formerly a sanitary operation, now inadvisable and dangerous, then it is plain that the experience of one who has practiced as much as twenty years not only does not add to his medical capacities, but detracts from them, making him really a dangerous man in society.

In this country no one questions the soundness of the public instinct, *i. e.*, when the strong current of public opinion runs in any particular direction, it is invariably in the direction of right—in the pathway of truth. It is true that when a proposition is first sprung, multitudes may give it their impassioned support, but a little time for calm reflection soon decides its fate. The same is true of all men, and if there has at all times been such decided opposition to blood-letting manifested by a large part of every community, then the strong presumption is that the public was justified in that opposition.

Dr. Gairdner says, “it may be freely admitted that the practice of the present day tends rather in the direction of that of the Pythagoreans and monks,” than toward the sanguinary practice of the Parisian Faculty in the time of Guy Patin, or in later times by Broussais and his disciples. He further admits that medical men have “no inducement to make a bigoted stand in favor of a remedy which is undeniably not in fashion,

one which they themselves now employ very rarely." In the examination of these declarations and admissions, I may be allowed to inquire why the medical practice tends to the non-sanguinary practices of the Pythagorean school? Is it because we know more of the science of human pathology, or is it because the type of disease has changed, or rather is it not the result of a more perfect understanding of the pathological states present in disease, and an appreciation of the modifications in the phase of disease? If, also, blood-letting is employed so "very rarely," as Dr. Gairdner admits it to be, then has the profession come to the acknowledgment of its injurious results, or do they withstand the temptation simply because it is "undeniably not in fashion?" If it is so "very rarely employed," then, in the name of common sense, why do Dr. Gairdner and his friends wage such a merciless war on Dr. Bennet? Dr. Gairdner admits that there never was a more favorable opportunity for banishing the practice than now, and he thinks that Dr. Bennet might have raised the standard of Van Helmont or Erasistratus, and have quietly floated down the stream of public opinion with as much certainty of reaching eminence, as by an attempt to revolutionize medical science. This all does very well for one who finds himself unable to answer the arguments of another, and therefore relies on the shafts of ridicule; but, in science, such proceedings have little weight. Besides, in all seriousness, if it had been such an easy matter to attain medical eminence by smoothly floating down the stream of public opinion, why should Dr. Bennet have undertaken the herculean task of revolutionizing medical science? That fact alone shows Dr. Bennet to be an honest advocate for medical reform, and I honor the man for the pains he has taken to expose the errors upon which the old pathology was founded, and, consequently, the old practice.

There is a circumstance connected with this subject to which I must call attention, inasmuch as it provides me an opportunity to inquire into the philosophy of the phenomena. This is what I refer to: The discussion of the theory of venesection, instead of *preceding*, has *followed*, the change in practice. Dr. Gairdner says that this is an "undeniable fact," and one which he says Dr. Bennet also admits, and I also add my testimony to its truthfulness. Now, this is the only answer that is needed to the question, why is this so? The practice of a physician is supposed to be founded on good judgment and a definite knowledge of the nature of the various diseases, and after he has been long engaged in practice it is a most difficult labor for him to admit his previous ignorance, notwithstanding he may have abandoned the errors of his practice. Hence, in the case of blood-letting, we find that after the case has become

obsolete, and one can hardly take advantage of another, then all feel willing to engage in a dispute in relation thereto, those having renounced it first being the leaders in forcing the discussion, while those who have but recently quit it are compelled to offer some sort of explanation, as in the case of Drs. Alison, Gairdner, Watson, Hastings and others. Dr. Gairdner, in his strictures on Dr. Bennet, which are almost too personal to answer his purpose, says that we are "only now beginning to inquire into its causes"—that is, the revolution which has taken place in regard to blood-letting. That may be true so far as Scotland is concerned, but in the United States this subject has been most thoroughly canvassed since 1825—over a quarter of a century ago. So thorough has been the discussion, so violent has been the opposition, that nearly a dozen medical colleges have been founded to break down the old practice; and now no section of country is destitute of its reform physician. The public has begun to understand the difference, and to discriminate between them when their services are required.

Dr. Gairdner takes Dr. Bennet to task for saying that this revolution has been brought about by "an advanced diagnosis and pathology," but certainly the former gentleman would not have the hardihood to deny that we have made very great advances in both our means and knowledge of diagnosis and pathology. The stethoscope and microscope have revealed the existence of many conditions which were previously unknown; and, therefore, Dr. Bennet is right in his position, it seems to me. Dr. Bennet and Dr. Gairdner are diametrically opposed in relation to abuses and the stethoscope, the former contending that as the stethoscope revealed the true nature of many diseases, it put an end to the abuses in the treatment thereof; which declaration, it seems to me, constitutes a self-evident proposition; but Dr. Gairdner asserts that it "led directly to their extension—a vast field of hitherto unnoticed diseases being brought to light." This declaration is so inconsistent with common sense, that I dismiss it without a comment. It will be understood by the readers of this volume that we have a class of physicians in the United States who term themselves eclectic, and whose characteristics have been drawn by Renouard, in his *History of Medicine*, in these words: "They drew from each (system) what to them seemed to be most conformable to reason and experience"—subjecting everything to the tests of common sense and experiment. The eclectic school in modern times has many characteristics that did not belong to that school in its earlier days. The modern eclectic not only claims affinity with whatever is true in the philosophy of any or all of the sects, but he claims, as a sequence of such im-

partial examination of all systems, that his philosophy leads to the only possible establishment of a homogeneous practice. He claims, and with good grace, that the constant comparison of rival systems enables him to perceive and lay aside his own errors of opinion—that in the bickerings of the sects, he, as a dispassionate judge, is enabled to decide the merits of questions in discussion. He is also led to the perception of principles not visible through the prejudices of sectarianism; hence, he claims the character of a discoverer, boldly advancing his attempts to render more positive and efficient the *materia medica* of the profession, and a more rational pathology. He claims that it is only by a comprehension of a rational pathology that any man can become a rational practitioner.

It is the aim of every department of a medical education to prepare the student for the exercise of a rational practice. A man may be a great anatomist, pathologist, physiologist, etc., but if he is not a master of general and special therapeutics and a good chemist, his practice will be irrational, and at most only experimental. The actual treatment of disease, is the desideratum in a physician's education, and he can never be a good, reliable and judicious practitioner if his practice is based on erroneous pathology, and he relies on uncertain agencies to effect those changes which must be induced to re-establish physiological conditions. Hence, every author of a work on practice should clearly set forth the basis of that practice by giving a rational pathology of every disease. This is usually attempted in the introductions to such works, but clearly, this practice is wrong, since the special pathology of particular diseases is thereby neglected; hence, it has been my aim to make this introductory chapter the vehicle for the discussion of general principles, reserving special pathological considerations for that portion of the work devoted to the investigation of special disease.

It is no longer denied by intelligent practitioners, that very great changes have recently taken place in the practice of medicine both in Europe and America. Nay, more, during the progress of this change hardly any two works on the theory and practice of medicine are alike, and certainly in the main, they do not correspond with the practice which we see the most eminent men engaged in propagating. It is also true that the young man who educates himself from books and one who is educated by lectures, differ as widely as do the philosophies of modern and ancient medicine. This schismatic tendency of European and American medical science, arises from the analytical character of the European and American mind, which leads us to ask questions, to demand proofs, to institute demonstrative experiments, to analyze propositions, to trace generali-

ties back to their special elements—to wage a ceaseless war on everything not consistent with reason and experience. Analogous circumstances present themselves in reference to our political systems, our domestic institutions, our general philosophies, and it is as true that we are passing through a definite scientific development, as that we are passing through a definite political development. A certain period in this analytic stage will usher in the syntheticism of the Asiatics, when we shall no longer ask questions, but simply use affirmations. This will be the result of demonstrative experiments, isolated cases of which are even now to be found, *e. g.*, every school and class of physicians *affirm* that the brain is the instrument through which the mind is manifested, yet that proposition is less than one century old. All physicians unite in affirming that ippecacuanha is an emetic, that quinine is a tonic, that iodine is an alterative, etc. These propositions have been clearly demonstrated, and our analyticism is completely satisfied. So, in time, will be almost every proposition in medical science; discussion leads to the establishment of these propositions, and hence, we have everything to gain and nothing to lose by legitimate discussion. I mean by the term legitimate discussion, such as is conducted with fairness, and where both parties are really desirous to elicit the truth. When such disputes, however, are stripped of the urbanities and courtesies that ought to characterize the doings and sayings of gentlemen, when they sink into personal reflections and disagreeable inuendoes, when the slang of billingsgate takes the place of dignified language and logical statement, then these discussions are productive of great evils.

CHAPTER II.

THE reader will not forget that I have already shown that Dr. Bennet is not the originator of the new pathological views which I shall here present, yet he may justly be considered the champion thereof in Europe at this time. While Alison, the venerable exponent of the olden pathology, will be readily admitted as the most consistent representative in Europe, yet we have been less impressed by his direct influence than through the labors of some of his pupils. In Europe the profession manages its discussions with more judgment and circumspection than we do in America. There the improvement is started in the medical society, while in the United States it originates in a closet and is first thrown out in a book or through the pages of a medical journal. The result is, the author is handled by the critics without gloves, and before he has had time to make a dispassionate reply, the controversy becomes personal and he is no longer able to do either himself or his subject that justice which both merit.

Were Dr. Bennet a professor in the University of Pennsylvania as he is in the University of Edinburgh, he would have been long since expelled, or had he been a member of the American Medical Association, as he is of the Medico-Chirurgical Society of Edinburgh, where his paper was read, he would have received his dismissal the following day, for if he is right, the great body is wrong, which, even though true, would hardly have been admitted.

As Dr. Bennet has defended the very positions taken by Beach and his disciples nearly thirty years ago, though sustained by different arguments, I take pleasure in presenting some of the more material points in his paper read before the Medico-Chirurgical Society of Edinburgh, on "the results of an advanced diagnosis and pathology applied to the management of internal inflammations, compared with the effects of a former antiphlogistic treatment, and especially of blood-letting." Prof. Bennet's paper was elicited by a paper from the venerable Alison, published in the early part of 1856, in which he undertook to show that the type of inflammation had changed. It is admitted everywhere among physicians of learning in the present day, that while it was formerly the rule, in the treatment of inflammatory diseases, to bleed early, largely and repeatedly—that now such bleeding is rarely practiced and is never necessary. This, remember, is the admission of Alison, the teacher of Watson, Hamilton, Fry, Hastings and other great names. Was it not for this very declaration that Beach was anathematized in America thirty years ago? Was it not Alison, Watson and similar authorities that were arrayed against Beach in this country? And is not the admission a clear vindication of Beach's observations? Is it not strange that, with such admissions from such men, the profession still make spasmodic efforts to sustain an old theory of Alison, when he, himself, admits its error?

Dr. Alison perceives the dilemma in which his disciples are placed, and with all the efforts of a great mind he comes up manfully to their rescue by asserting that his theory *was* true, but that the type of inflammation, and especially of the accompanying fevers, have changed from inflammatory to a typhoid character. I have already shown the absurdity of this proposition, and shall pursue it further through these pages. This proposition is set up with apparent candor by Dr. Alison, to defend the old therapeutic rules; for if it be established that those rules were wrong, then Dr. Alison has been in error all his life, and what he may have written is merely a record of that error. Dr. Alison must have seen this, and hence, to brace up his position, he has been compelled to use the affidavits of his old pupils.

Dr. Bennet lays down five propositions, from which he proceeds to reason. These are, 1. That little reliance can be placed on the experience of those who, like Cullen and Gregory, were unacquainted with the nature of, and the mode of detecting internal inflammations. 2. That inflammation is the same now as it has ever been, and that the analogy sought to be established between it and the various types of essential fevers is fallacious. 3. That the principle on which blood-letting and antiphlogistic remedies have hitherto been practiced, is op-

posed to a sound pathology. 4. That an inflammation once established cannot be cut short, and that the only object of judicious medical practice is to conduct it to a favorable termination. 5. That all positive knowledge of the experience of the past, as well as the more exact observation of the present day, alike establish the truth of the preceding propositions as guides for the future.

If Dr. Bennet had admitted, as no doubt he will, eventually, that the *phase* of inflammatory disease has somewhat changed, his propositions would only require re-statement to repeat the principles on which the eclectic branch of the medical profession rests its claims to respectful consideration.

Until recently, inflammation has been recognized by pain, heat, redness and swelling, especially when external, and internally when fever was present, accompanied by pain and impeded function of the organ affected. The nosological systems were purely artificial, and as these chanced to vary, so the essential characteristics of inflammation were made to vary. This led to confusion and endless error; but the researches of morbid anatomists have shown that inflammation is never present except when the inflamed part is in a morbid condition—that it is a result of such morbid derangement; that it is a symptom; that it results from a concentration of the plastic power; that it is not a disease *per se*, but the result of an extraordinary effort of the system to maintain its integrity; that it is the natural result of organic lesions, and in treating it we shall regard it as symptomatic of a morbid state at all times, as will be clearly shown in an examination of the points of consideration introduced by Dr. Bennet.

Dr. Bennet says “the school of morbid anatomy, by showing that inflammation was a diseased *condition* of a part, entirely overthrew the errors and confusion inherent in all such nosological systems.” The actual observation of physicians by the patients’ bedside, aided by the increased facilities of diagnosis and correct pathology, has demonstrated beyond a rational doubt that the nosological group of symptoms formerly supposed to indicate internal inflammation, bears no relation thereto, in fact; and these investigations have further developed a large mass of information that has never yet been systematized, but is scattered here and there through the writings of various modern physicians. This is a position taken by Bennet, and, to my mind, it is well founded. Now, in any sense, can inflammation be called a *disease* or not? It certainly is not a disease *per se*, but it is a morbid *condition*. It is a symptom of a morbid state, and may be called a disease, or regarded as one; in fact, if we only bear in mind that histology has exhibited to us the fact that inflammation is a disease of nutrition, governed by the same laws that determine the growth and

function of cells—*i. e.*, what Dr. Draper calls the plastic power—and that in this sense physiology and pathology are united into one science that has enabled us to get still further out of the traditional errors of the past.

From this statement, Dr. Bennet asks the significant question, “Why, then, should we, in our onward course, be governed by the opinions of Cullen and Gregory, of Gabius and Sydenham, of Aretæus and Hippocrates?” Certain it is, that while those distinguished men stood at the head of their profession in their day and generation, yet the imperfections of science were such that they would commit many very great mistakes; and because they were great and learned in their day, shall it be said that we, with all the lights of the nineteenth century, are bound down to their errors simply because they have the sanction and authority of the antique? Absurd as it may seem, this is just what Dr. Alison and his followers would have us do. With equal propriety, navigators ought this day to be compelled to act on the exploded astronomical notions of Tycho Brache and Copernicus, which would be simply ridiculous.

Those who have heretofore written on medicine, have treated the errors of olden physicians with too much consideration, always feeling that they must disprove the old theories and rules before they should advance the new ones. The amount of labor thus uselessly expended has been very great. Now, it is apparent that the establishment of the new theory—the advancement of the more demonstrable principles, would, in the very nature of things, have consigned the old doctrines to merited oblivion. It is for this reason that I shall not enter into any lengthened exposition of the errors of the leaders of the profession, from Hippocrates down to Alison. Alison, however, living late enough to observe the war of the new on the old pathology, may yet correct his previous teachings, and thus re-instate himself in the van of his profession.

By inflammation, is meant *an exudation of the normal liquor sanguinis*. This is what Bennet understands it to be—it is what Alison seems to understand it to be when, as Bennet says, he acknowledges “that exudation of lymph is essential to almost all changes of structure produced by inflammation.” Bennet again says, “both Dr. Alison and myself (by the term inflammation), mean a change in a part characterized by the exudation of lymph through the walls of the minute vessels, resulting from changes more or less well marked in the nervous, vascular, sanguineous and parenchymatous elements of that part.” To this I fully agree.

Diagnosis will vary in correctness—as a basis of opinion, as our knowledge of special and general pathology is more or less perfect. Thus, until recently, internal inflammation was

never sought after except through the manifestation of symptoms. But our modern investigators, who have pushed their inquiries to a very great extent, show most conclusively that symptoms are not to be relied on alone, since, where all the symptoms of inflammation have been present, yet *post mortem* examination has demonstrated the absence of the lesion. Again, inflammation has caused many deaths, without one of the symptoms supposed to always accompany it having been present. This is admitted now, by even those who endeavor to bolster up the old practice. We know, and as Dr. Alison admits, that we can now detect the presence of inflammation of the lungs where, in days gone by, we should have never dreamed of the existence of pneumonia, and simply because there was neither pain, cough, dyspnoea, or inflammatory fever present. But, with the stethoscope—with the light thrown out by the researches of morbid anatomists—we now detect the presence of the inflammation, even though complicated, as it usually is, by other phases of disease. Dr. Alison thinks that cases thus overlooked were attended with very little immediate danger; but this assertion is certainly not well founded, since, even now, the latent pneumonias are the most fatal, and, as Dr. Bennet says, “must always have been so.” Again, the pain, cough, dyspnoea, rusty sputa and fever, which are, or have been, regarded as characteristic of pneumonia, are met with in various other morbid states, and where inflammation of the lungs is not present; and Dr. Bennet mentions as illustrations the bronchitis of young persons, the engorgement and apoplexy of the lungs, associated with fever or heart disease in older persons. It will, therefore, be seen how many a patient was previously bled for a pneumonia when none was present, and how, in other cases, blood-letting was not practiced when pneumonia was actually present. It is said by many advocates of the old practice that Pneumonia is not now what it formerly was. I admit that the phases thereof are somewhat modified, from causes already enumerated; but does not the difference consist in our more perfect acquaintance with the disease, and greater facilities of diagnosis, than in any actual change in the type of inflammation of the lungs? I admit that we call certain diseased states pneumonia in 1859, which in 1759 were either unnoticed, or referred to some other tissues than the lungs; and the reason of this is plain enough. Then, if so little was known, comparatively, by the older physicians, how shall we be enabled, from their experience, to treat disease in 1859 rationally, since we know the disease not to be what it was formerly thought to be? The only way in which the experience of the past can be useful to us is by bearing constantly in mind what was really known, and from the practice of ancients, and its results, we shall be enabled to glean some-

thing of the action of medicines on diseased conditions which the ancients did not really understand. We have to deal with living men in the present day, and hence our studies should tend to the investigation of the diseases affecting men now, rather than in commenting on, and following the opinions of men in other ages, whom we know to have been destitute of much that is now known. We must examine diseased conditions as we find them, and this fact is apparently admitted in the desire shown by so many authors to publish their own experience in the healing art. It is only now that we are beginning to be able, with the special researches of the most enlightened men, to judge correctly of the value of old dogmas, and to confidently advance new rules and practices. Experience is worth something only as the experimenter has comprehended the subject of his experiments; or, in other words, the experiences of one who has had the advantages of "an advanced diagnosis and pathology," as Dr. Bennet says, is evidently worth more than one who has had no guides better than the theories of Hippocrates.

It has already been stated that a series of changes in the nervous, sanguineous, vascular and parenchymatous functions, terminating in exudation of the liquor sanguinis, constitute the essential nature of inflammation. Dr. Bennet, in the argument of his second proposition, asks, "how it can be shown that any of these necessary changes have, of late years, undergone any modifications?" *e. g.*, if a healthy man receive a blow, a cut, a bruise, or any other injury to his person, are the resulting phenomena in any sense different from what they were in the time of Hippocrates, Galen, Cullen or Gregory? There is a difference only in the intensity of those phenomena. Bennet asks: "Are the effects which followed wounds received at the battle of Alma different from those which resulted from similar injuries at the battle of Waterloo? This has not yet been shown." Again, in our hospitals do we observe phenomena which were not presented in similar cases many years ago? Or do we find that the phenomena described by Hippocrates are no longer seen under the same pathological states? If a person expose the feet to damp and cold, and get an inflammation of the lungs, do we not observe hepatization now as formerly? And if it be admitted that we do have the same hepatization now as in the days of Cullen, is it not removed in the same way that it formerly was? Now, then, if these changes cannot be shown, as they cannot be, then I ask for the proof that the essential nature of inflammation has changed within the last thirty years, so as to require the complete revolution in practice which we observe, both in this country and in Europe, and which is no longer denied by any physician of respectable intelligence.

The only possible answer which can be made is that which has been proposed by Dr. Alison: that the symptoms of pneumonia given by Cullen differ from those given by Grissolle, and other recent writers. This answer is exceedingly imperfect, since it has only been since the days of Cullen that physicians have learned that the symptoms of pneumonia given by Cullen do not necessarily arise from pneumonia—that they may exist where there is no inflammation of the lungs—and that pneumonia may exist where none of these symptoms are present. The stethoscope, together with the researches of morbid anatomists, has revealed symptoms not known to the older writers, and hence the change of given symptoms. It is for this reason that we can draw no inferences as to the changes of the types, or even phases, of disease now, as compared with the same diseases in the time of Cullen and Gregory. The symptoms of pneumonia then constituted the disease, in their estimation; but now we seek for the lesion which is the basis of the disease, and hence, too, we now find actual pneumonia present where Cullen never would have suspected it; or, again, what he would have pronounced a pneumonia, we now know, often, not to be really such.

In consequence of refusing to recognize these facts, Dr. Alison says, inasmuch as the fever which accompanied Cullen's pneumonia was inflammatory, and that which now accompanies pneumonia is typhoid, that, therefore, the pneumonia of this day does not bear blood-letting as did the pneumonia of Cullen. To this Bennet replies, and my own experience confirms his statement, that, during seven years' experience as Physician to the Royal Dispensary of Edinburgh, he has had abundant opportunity, and has satisfied himself that a true pneumonia is the same everywhere, and under all circumstances. When it attacks old, debilitated patients we may expect it to be typhoid; and when it fastens on young, vigorous persons, it will be inflammatory; and this, no doubt, has always been the case.

As it is generally admitted that fevers do present changes in type, or rather in phase, and Dr. Alison contends that, therefore, inflammation may also change, Bennet has shown, in the paper to which reference has already been made, that essential fevers may be either typhus, typhoid or ephemeral; but this variety of phases depends altogether on the variations of intensity of the exciting causes. What the nature of these variations is, is not clearly established; yet from observations in this country, and from the observations of scrupulous observers in Europe, I think that these variations depend on diet, locality, climate, meteorologic influence, etc. But the causes of inflammation are the same everywhere, and the only variation arises from the constitu-

tional stamina, and the intensity of the cause. It is assumed by the advocates of the change theory that the pulse, too, has undergone a change, and is not now full and bounding, as it formerly was,—to which Bennet replies: “For my own part, I have earnestly sought for, but cannot discover a shadow of evidence for such a belief. Moreover, I have a most lively remembrance of all the facts and circumstances connected with the bleeding of many patients by myself, twenty-eight years ago, when I first commenced the study of medicine, as well as of such as took place in the Royal Infirmary of Edinburgh, when I was a student in the University in 1833; and my impression is that not the slightest difference exists between the character of the pulse now, and what it was then.”

In British India, in the United States, in France, in Germany, the practice of blood-letting is as little practiced as in Great Britain, and it would be fair to infer that where these changes in practice have taken place that, on the theory of Dr. Alison, the type of disease has changed. But in Italy, and in the Charity Hospital in Paris, physicians bleed now with as much heroism as at any previous time, and we must necessarily, on Alison's theory, conclude that diseases have changed in India, America, Germany and Great Britain, but that they have not so changed in Italy; or that, while they have changed everywhere else in France, they have not changed, in Bouillard's judgment, at La Charite Hospital. Such a belief would be unworthy of Dr. Alison, and many of those who have undertaken to defend him against Dr. Bennet. These facts go very far, in my mind, toward establishing the second proposition of Dr. Bennet—that inflammation in a part is the same now as it ever has been, save those variations of habit, diet, etc., which have been already pointed out.

The profession has so much at stake in this question of change, that I consider it my duty to establish one or the other proposition—change or sameness—at the very outset of this treatise. This is the first attempt, in America, to present the subject in detail, and if I shall settle the question definitely, I shall have attained a point from which every practitioner can judge the rationality of the numerous sectarian theories that now divide medical opinion. The ready adoption of old theories, merely for antiquity's sake, has been prolific of very much error in medical practice.

It has been supposed that almost every disease depended upon the presence of morbid materials in the blood, and bleedings have been practiced to remove these morbid materials. Again, blood-letting has been practiced to diminish the flow of blood to a particular part, where the seat of disease was supposed to be. That there was an accumulation of blood in the part so diseased, and that general blood-letting would re-

lieve the part of such accumulation, and from all other facts, it has been determined that the pulse was the index as to the quantity of blood that ought to be drawn.

But before adopting these rules of practice we ought to have first determined whether these rules really had any foundation—whether they were really supported by facts. If, in general blood-letting, it can be shown that the *materies morbi* are alone removed, then that fact would very strongly point to the philosophy of venesection. The great difficulty of the older physicians consisted in confounding fever and inflammation. The old idea was that the circulating fluid was thrown into a sort of ebullition, that worked off the morbid materials; that if this morbid matter was allowed to remain in the blood, it finally fell on some internal organ, and produced inflammation. It was thought that the blood was diseased first, and that local lesions supervened as a consequence. These views are found in the writings of the advocates of blood-letting, from the days of Hippocrates to those of Sydenham; from him to Cullen, and even down to the present day.

While it will be generally admitted that the system is more or less influenced by the most insignificant local disease—for nutrition in every part depends on the blood, and whatever modifies the nutrition of any part, either to stimulate or retard the process, will, therefore, to that extent alter both the quantity and the quality of the circulating fluid—yet it seems to have been forgotten that each disease is itself the means for expelling this morbid material. Dr. Addison has pointed out this in distinctive eruptive fevers, as small pox, where the small abscesses in the skin eliminate the poison which formerly existed in the blood: and it is by favoring that elimination that we may expect to effect a cure. Experience shows that we cannot effect the same end by general blood-letting. So true is this that almost every well informed physician now admits the fact, and modifies his practice to the accommodation of experience rather than of the theory.

The same is evidently true of inflammation, when, in addition to the local changes in a part, there must necessarily be more or less disturbance of the general functions of secretion and excretion. In all these changes the blood is materially modified. The secretion which is suppressed is, of course, accumulated in the blood, along with an excess of fibrin. The investigations of Andrel, Rodier, and others (in fact, *no one* questions the statement), have shown that venesection greatly deteriorates the blood, impoverishing it in its red corpuscles, and enriching it in water; but it is also a fact that the proportion of fibrin is not so affected. That there are *materies morbi* in the blood is not denied, but these can only be removed by cell development; and that peculiar vital chemistry which governs

the character and movements of the blood. Blood-letting retards both cell growth and chemical elaboration in the animal economy, and for this, if for no other reason, it ought not to be practiced.

Again, it may be inquired, whether a correct understanding of general pathology justifies us in retarding the flow of blood to an inflamed part. The increased flow of blood *to* an inflamed part, and the increased throbbing, are not the cause, but the result of the inflammation. Dr. Williams, in his *Principles of Medicine*, treats the "determination" of blood to a part as a *cause* of disease; but hardly any well informed physician would now be willing to stake his professional reputation on such an apparent error. Dr. Bennet says: "In all cases, the primary stimulus producing inflammation is applied to the vessels of the part, either directly (as from injuries or irritants), or indirectly, that is, by reflex action (as in the case of internal inflammations, following exposure to cold, etc.); and, in consequence, that is to say as a *result* of the local change in the part thereby occasioned, there follows the throbbing of the neighboring arteries." And Prof. Bennet then proceeds to illustrate this position by calling attention to the circumstances of a thorn driven into the finger. Here the irritating body acts on the nerves and blood-vessels of the part; stoppage of blood and exudation follow, and lastly we have the throbbing of the artery in the finger. Now, then, the throbbing is the evidence of the so-called determination, the cause of which has been the injuries inflicted on the nerves and blood-vessels of the part by the thorn. The throbbing is a result of the inflammation, and by no means a cause of it. And Dr. Bennet asks the significant question, "whether would it be reasonable to treat such an inflammation by opening the artery, or by favoring the transformation of the exudation thrown out into pus, whereby the irritating cause and its results are both got rid of?" Every physician who has had any experience knows that the latter course would constitute the proper treatment, and that the wounding of an artery under such circumstances would be the most unpardonable mal-practice. Dr. Addison, in his cell therapeutics, points out very clearly why there should be an increased flow of blood to a part that is inflamed. Inflammation having been established, it is necessary, in order to break up the exudation, that there should be an increased growth by cell formation; there must be sent hither an increased flow of blood, whereby the whole shall be eliminated from the economy, either by external discharge, or by being taken into the general circulation, and then expelled by the natural emunctories. It is a common law, and universally admitted in the science of physiology, that, where there is an increased growth there is an increased flow of blood—a good example of

which is seen at the period of heat in animals, when an increased flow of blood is required to ripen the graafian vesicles. So, too, in the scalp of the stag during the development of the antlers; in the female mammæ when milk is first secreted; in the gums of an infant during dentition; in the case of tumors within or on the body; and in the ascent of an increased flow of sap in the plant, when its foliage is being developed, in the spring of the year. In all these instances the increased flow is drawn to the part in consequence of the increased production of cells, which necessarily require an unusual amount of blastema. So says Dr. Bennet, in inflammation, an exudation having been poured out, which has to be transformed by a process of cell growth, in order that it may be removed, or rendered subservient to the wants of the economy, it is absolutely imperative that the part in which these nutritive changes go on should receive more blood, to enable it to accomplish them. This, then, is why Nature, in her wisdom, directs an increased flow of blood to an inflamed part, and when it is so understood, no rational physician will entertain for a moment a wish to arrest that flow. Yet the great mass of the profession has, hitherto, regarded this increased flow as positively injurious, and they have racked their inventive powers for means to arrest this sanitary current. Blood-letting has been, as Sydenham says, their "sheet anchor;" but now all can see on what false pathological data.

This is the doctrine for which the eclectic branch of the medical profession has so manfully battled during the past quarter of a century, and which Dr. Bennet now brings forward in Europe as a thing quite new. I care not who has the credit therefor, but I beg to call attention to the fact that the natural researches of physicians, when understandingly prosecuted, lead spontaneously to the same conclusions. Dr. Bennet has put a flea in the ear of the European profession, and no human power can ever crush the bold thought which he has uttered.

It will be borne in mind that I do not contend that all who have claimed to be eclectic have built their practice on these views. Far from it; for there are many in this branch of the profession, as well as in other branches, who will sooner follow the practice of others on the basis of general results, than take the trouble to inquire *why* their practice differs from the rest of the profession. Nay, more; we have large tomes on the practice of medicine, eclectically, by men who are shamefully ignorant of the first elements of true eclectic practice. The leading eclectic school in the United States has often endeavored to shake off such, and, to a very great extent, has succeeded; *e. g.*, the large number expelled by the Board of Trustees of the Eclectic Medical Institute in 1856. Rather than admit their superficialities, they actually organized a school in order

to maintain their respectability in the profession ; but the ruse was a failure, and in less than two years only one or three remain in these parts. It has been from the unwise course of such persons that the general profession in this country has been so prone to keep aloof from those whom they knew to be physicians of the very highest attainments. But, since there was never yet a medical, religious or political organization, clear of these funguous attachments, it is to be hoped that this branch of the medical profession will not materially suffer thereby.

The next important question in the discussion of these propositions is, does general blood-letting diminish the amount of blood *in* the part which is laboring under an inflammation? It would be absurd to deny that general blood-letting in the treatment of inflammation has long been practiced with this intention. It has been believed that the part ought to be freed from sanguineous engorgement, and that blood-letting would accomplish that end. Admitting that it were desirable to diminish the quantity of blood in the part, it certainly cannot be shown that blood-letting will do this directly. It has never yet been shown that draining the general system will relieve an engorged part. The practice and principle are founded on the idea that the blood has a peculiar affinity for the inflamed part, and not that it is merely arrested. The blood corpuscles in such a part are closely crowded in the enlarged vessels, and the vascular tissue is thus distended, which phenomena are in no way affected by the flow of the arterial current, even though in close proximity. Now, the removal of the exudation is admitted to be the great aim of treatment; and I ask how, when this exudation is outside of the vessels, can general blood-letting alter this state of affairs? No local or ordinary general blood-lettings can directly diminish the quantity of blood in a part, and even when large quantities are abstracted the effect is indirect, and could be much more easily obtained, if it was actually necessary, by bandages on the thighs and arms, which, by compressing the veins and not the arteries, would enable the latter to pump the veins of the limbs full, and thus diminish the general amount of circulating fluid. This plan has been proposed, but there is as little indication for that practice as there is for blood-letting. The phenomena of syncope can be thus much more readily induced than by venesection, but nothing more is gained save that the system is not so impoverished. But it must be borne in mind that if the blood be thus dammed up for any considerable time, that it will be unfit for circulation in consequence of the very large accumulation of matter in its descending metamorphosis; a fact apparent enough when we remember that for every beat of the pulse no less than twenty millions of blood-cells are destroyed, which must be speedily eliminated or general irri-

tation and fever supervene. It has been already seen that the exudation can only be removed by new cell growth, and that this depends on an increased flow of blood; therefore blood-letting, by arresting this increased flow, would retard the removal of the exudation, and thus prolong the effort of the system to recuperate.

Local blood-letting is altogether inexplicable, and on no theory can it be shown to effect any real good. If a person is attacked with pneumonia, and leeches are applied to the integuments, how does this affect the changes in the vessels supplied directly from the aorta, since the integuments are supplied by vessels derived from the mammary arteries? There is no direct anastomosis between them as has been demonstrated by Struthens and other observant anatomists; yet it is not denied that local blood-letting often removes the pain incident to such inflamed parts, and the *rationale* thereof is demanded. I ask if the good effects do not depend upon the reflex influence of counter-irritation, or from the soothing influence of the warm fomentations which are at the same time applied? It is a known fact that dry cupping is often as efficient as local bleeding, and if the explanation is other than that already given, why is this so? No reasoning and no experiments have yet demonstrated that local blood-letting can diminish the quantity of blood in an inflamed part.

Everywhere, and for nearly all time, the advocate of bleeding has rushed to the wrist to examine the state of the pulse, without, in all probability, ever once inquiring seriously whether a strong pulse necessarily implies a necessity for the abstraction of blood. Or, as Dr. Bennet says, "because nature accelerates and strengthens the pulse, it has been thought that art ought to interfere and diminish its force and frequency." It has been already shown that nature rids herself by the formation of new cell growth to transform the exudation of inflammation, and finally eliminate it from the system, and for this reason she raises the force and frequency of the pulse. The object of interference is to lower the pulse—to diminish the force of the circulation—to retard the flow of blood to the part, and therefore the effect is to prolong nature's effort to secure the necessary transformation. The inconsistency of this practice is readily seen when the practitioner, finding the pulse hard and full in pneumonia, at once bleeds the patient until it becomes weak, and then he immediately sets about strengthening it by the administration of tonics and stimulants. This contrary practice is proof of the error in one or the other case—either that the man's blood ought to have been let alone, or else, when drawn, he ought to have been allowed to remain weak. If it was right to weaken his pulse, why strengthen it after that end has been attained? Dr. Bennet is so clear on

this point that I quote his language: "In short, we argue that the phenomena of fever and excitability following inflammation, have been wrongly interpreted, and that danger is to be apprehended from them—not directly, but from the subsequent exhaustion which all great exertions of the animal economy produce. In themselves, these are sanitary, and indicate the struggle which the economy is engaged in when attempting to get rid of the diseased process; and we only diminish the chances of that struggle terminating favorably by lessening the vital powers at such a critical juncture."

It is, however, contended that blood-letting relieves pain, and therefore ought to be practiced. That it does relieve pain is a fact, but it must be admitted that the duty of the physician is to cure and relieve. He tries first to cure, and if his attempts are abortive, then he tries to relieve. The principle of cure and the principle of relief may be, and often are, incompatible; in which case the principle of relief should be sacrificed to the principle of cure, as when that has been accomplished the relief will be spontaneous, but the relief does not imply a spontaneous cure by any means.

Blood-letting seems to be advisable only in the case of aneurisms, yet, generally, I doubt whether even then it prolongs the patients life a single day. It is true that the expected relief will be obtained by a small abstraction; yet, when frequently practiced, as it must be, the system is so weakened that, in all probability, the patient sinks under the treatment sooner than he would from the disease. But it is not for the sake of palliation that blood-letting has been heretofore practiced, and even when it is so practiced, the philosophy thereof has not been commonly understood. And from what has here been said, it is evident that the principles on which it has been heretofore practiced are erroneous, which is the position taken by Dr. Bennet in his third proposition. If the facts adduced can be refuted, every physician ought to feel it a duty to come forward with the arguments and data of refutation. No such attempt can be successful, however, in my opinion, since truth cannot be rectified.

Dr. Bennet's fourth proposition is, that an inflammation once established cannot be cut short, and that the only end of judicious medical practice is to conduct it to a favorable termination. This is a bold proposition, and if clearly established, must weigh very heavily against the antiphlogistic treatment of inflammations. By a little examination of the older authorities, we shall find that they held it to be the duty of the practitioner to interfere in the treatment of many diseases which are now allowed to run their natural course, as typhus, small pox, etc. Interference in the treatment of these complaints was attended with such fatal consequences, that

physicians were compelled to disregard authority and appeal to experience for principles of procedure. In this way it has been agreed on all hands that the duty of physicians in the treatment of these diseases and many others, is to prevent their attachment to the system if possible; and, if this fails, then to conduct them to a favorable termination. Internal inflammations run through a definite course as well as those complaints, and if so, then I can see no reason why the same principles of practice should not pertain in this as in that case. To comprehend the fact of definiteness in the progress of an internal inflammation, we have only to examine the phenomena. By closely observing the course of an inflammation, we notice that it terminates in one of two ways, first, in the language of Bennet, by vital changes of growth of different kinds in the exudation, constituting what has been heretofore called supuration, adhesion, granulation, cicatrization, the healing process, etc.; or, secondly, by death of the exudation, which, if rapid, putrefies, producing gangrene; or, if slow, disintegrates, causing ulceration, suppuration, adhesion, granulation, etc.,—really sanative processes. Especially is this so in regard to suppuration, which ought to be regarded, as it really is, as a growth that enables the coagulated blood-plasma and exudation to be broken up and eliminated from the system. It should, therefore, be encouraged; yet it has hitherto been the aim of the practitioner to check rather than encourage the process. It is evident that whatever weakens the strength or debilitates the patient, will retard the nutritive processes of development, and thereby impede exudation. Blood-letting being thus capable of weakening the system, is directly opposed to the rapid breaking up of an inflammation; and therefore would not be employed by a judicious practitioner well informed of the pathology of inflammation.

That inflammation is a sanative process, needs only to be understood to be acknowledged. Thus, if a splinter be thrust into the finger, what is the effort of nature to remove the foreign substance? Inflammation, suppuration, etc., there being a regular series of changes until the splinter is finally expelled. If it was a destructive process, nature would hardly invoke its aid in a recuperative way. If there had been no inflammation, there would have been no “festering” and no expulsion of the splinter. Again, if a bone be broken, inflammation in the region of the fracture is set up, an exudation takes place, and by certain vital processes this exudation is finally transformed into bone. If the surrounding soft tissues are injured, there is poured out an exudation from the adjoining vessels, which, by other vital changes, is finally transformed into equivalent soft tissues—fibrous it may be, constituting granulations first, and cicatrization afterward. When a vio-

lent blow has been received, more or less exudation occurs, and is infiltrated among the bruised tissues, which, by cell growth, is transformed into pus; which, if an opening be made, is got rid of externally; or else, by the disintegration of the cells, is absorbed, and is finally eliminated from the economy. In the one case—that is, where the pus is absorbed as fast as it forms—the tumor is said to be discussed; but if not, it collects in a body, a fluid mass, and then constitutes an abscess. Now, if these processes are recuperative and essential, how can blood-letting favor them, when it is universally admitted that the general system is weakened by the practice? When called to treat a patient who has suffered a fracture, a bruise or an injury, if we find the constitution strong and vigorous, our prognosis must be, and always is, favorable; but if the patient is scrofulous or very weak, we know that recovery will either be slow or impossible, and hence, the unfavorable prognosis of the case. Yet, while this principle is recognized and acted on in practice, we find numerous respectable physicians contending for a theory that would lead to directly contrary results. This is one of many medical inconsistencies.

In the case of internal inflammations, the processes are identically the same. Suppose the part so attacked with inflammation to be the lungs or pericardium, do we not have the exudation converted into pus cells and absorbed, or into fibrous texture forming adhesions? No pathological anatomist will deny this statement; yet, instead of witnessing the physician treating an internal inflammation as the surgeon does an external one, he attacks the general symptoms, which are but the effects of a lesion. In other words, he endeavors to gag nature when she tries to proclaim her ailment by certain symptoms. He combats their appearance, and endeavors to drive them back without once striking at the cause which produces them. In the case of a fracture we have febrile symptoms as a result—the pulse is increased, etc.; yet would any well informed surgeon venture to affirm that callus would more readily form by blood-letting and antiphlogistic treatment? Experience has taught the surgeon otherwise; and as internal inflammation is the same, why should the physician not profit by the same experience?

CHAPTER III.

IN the case of pneumonia, we have what is called hepatization of the lungs, which is only the completion of the changes which serve to make up a true pneumonia. "The exudation is infiltrated," says Dr. Bennett, "into the air vesicles and minute bronchi, and between the fibers, blood-vessels and nerves of the parenchyma, imprisoning the whole in a soft mass, which coagulates and renders the spongy texture of the lung more dense." This effectually prevents the entrance of the air, arrests the circulation, and compresses the nerves. Nature, in order to re-establish her functions, labors to re-convert the solid exudation into a fluid, that it may be evacuated from the bronchi, or re-absorbed into the circulation, and finally be thus expelled from the system. This is accomplished by cell growth entirely, in the formation of pus. Having been thus reduced to pus, it is taken into the circulation, where it is once more metamorphosed—changed into urate of ammonia, or an acid, etc.—and in its further changes passes into urea and carbonic acid, and a number of other chemical combinations. If the amount of oxygen taken in is insufficient to effect these final changes, it appears in the urine as uric acid and fibrin. Liebig gave us the first clue to these chemical changes which occur in the system while the pus is being prepared for elimination from the economy.

In pleurisy, pericarditis, etc., the changes are somewhat modified, but the same great principles apply in the one, as in

the other case, and the final results are identically the same. In every instance we find an increased flow of blood, and a heightened cell growth. So true is this, that in pericarditis, where there are no cells to bring in the increased flow of blood, they are formed especially for the occasion. Transformation is the great object, and as this depends on the increased flow of blood, in what way can venesection prove beneficial, when it is admitted to weaken the general system?

Having satisfactorily disposed of these propositions, and answered the objections thereto, I next proceed to canvass the fifth proposition of Dr. Bennett, which appeals to facts and experience to determine whether the principles already advanced are sustained or not; and here let me say, every practitioner into whose hands this may come, ought to ransack his own memory, and thoroughly scrutinize the subject so as to come to a rational conclusion. Twenty years ago, the advocates of blood-letting selected pneumonia as that disease which particularly demanded the assistance of the lancet; hence, since, also it is so held up even to the present day, pneumonia should now be selected as that disease which should be, in its treatment, the basis of the facts to be adduced. But before the value of any practice can be determined, it is necessary to learn what is the natural duration of the disease. In the homœopathic hospital of Vienna, where, though the patient takes certain little pellets—perfectly inert so far as medicinal action is concerned—the patients were cured, if at all, by nature, Dr. Balfour watched closely the progress of the disease, and any facts derived from that quarter will be of great value to us, as studies of the disease when left entirely to the operations of nature; for, as Dr. Bennett well says, “no sane man regards homœopathic medicament as anything else than inert,” and, I may add, but a blind, by which the physician gets paid for watching nature cure a disease. There is also extant the recorded experience of Skoda and Dietl on the expectant systems, and from all the facts thus elicited, it appears that where the attack is slight the patient may be convalescent on the seventh day; that the majority of cases of medium intensity get well between the seventh and fourteenth days, and very severe cases recover between the fourteenth and twenty-first days. Authors differ, however, as to the period of the commencement and termination of the disease, as they regard this or that symptom of more or less importance. The rule most commonly adopted is to commence the count from the day of the first rigor, and continue it until the leading symptoms have ceased. It is very true that the sequelæ, as it were, of these symptoms may last for some days after the patient has left his bed. Dr. Bennett is of the opinion that when two-thirds of one lung is involved, if the recovery takes place

in fourteen days, it is a good recovery, but not better than ought to be expected in a healthy constitution. It must also be remembered that the intensity and duration of the attack do not of necessity bear relation exclusively to the amount of the lungs involved. We see one person, with only a part of one lung involved, laboring under a very high fever, and with intense symptoms, while another, with the whole of one, or a large share of both lungs diseased, has very little constitutional disturbance. In the former case, where the symptoms are very severe, the recovery may be rapid, and in the latter, where the conditions are reversed, we shall generally find the recovery slow and tedious. These circumstances are only recently understood. The disease is now understood to be an extraordinary, but natural effort of the system to rid itself of foreign substances, to maintain itself intact from the ravages and effects of irritation and its results. The effort which the system is thus enabled to put forth will be proportioned to its vitality—its health. In a child with a healthy stomach, we observe the immediate expulsion of poisonous and improper substances, while in the case of a weak and sickly child the case is far otherwise. The fevers of the one are intense, and rapidly run their course, while in the other they are of a low grade, and slowly linger on for weeks, and it may be months.

The common belief has heretofore been that the symptoms of pneumonia constituted the disease, and as these might be controlled somewhat by bleeding, it was thought that venesection was of all other means the proper treatment. It is now known that the smothering of the symptoms in reality only disenables the system to perform its work of recuperation; that the disease, even after the suppression of the symptoms, will linger on for weeks, giving rise to a very tedious convalescence, which may destroy the patient by general exhaustion. The relief of symptoms, then, let it be understood, is quite another thing from the cure of the disease, and we must pronounce that the best practice wherein the fewest deaths occur, and under which the duration is shortest. Now to arrive at something like satisfactory conclusions, let us look at a few statistical facts, which are mostly derived from the paper of Dr. Bennett. In ten years—from 1839 to 1849—there were admitted into the Royal Infirmary of Edinburgh 46,965 patients, of whom 648 were affected with pneumonia, and under the blood-letting treatment 222 died, while 38 only were relieved. Thus over one-third who entered the Infirmary for treatment died, and this proportion will hold good, on the authority of Dr. Bennett, for any time since the commencement of the present century. The like astounding mortality was observed by M. Louis in the Hospital of La Charité, at Paris. Dr. Thornburn found in 208 case books of the Edin-

burgh Infirmary, running from 1812 to 1837, belonging to twelve physicians, all of whom practiced an antiphlogistic treatment, 103 cases of pneumonia, of which 55 were cured, 41 died, and 7 were relieved more or less. But to be still more sure, Dr. Thornburn carefully read over these one hundred and three cases, and rejected such as did not present the most unequivocal evidence of pneumonia. The result gave him 50 cases of pure pneumonia, of which 31 were cured or relieved, and 19 died!

M. Louis (*Reserches sur les effets de la saignée*) records 107 cases, of which 32 died—a mortality of 1 in $3\frac{1}{2}$. Of 78 cases occurring at La Charité, bleeding was performed from the first to the ninth day, and the deaths were 28 or 1 in $3\frac{1}{4}$. The average duration of the cases that recovered was $15\frac{1}{2}$ days. In 29 cases observed at La Pitie, the bleeding was performed during the first four days, and the deaths were only 4—or 1 in $7\frac{1}{4}$; but it was observed that in those who recovered the duration of the disease was $18\frac{1}{4}$ days. From the diminished mortality, M. Louis thought that bleeding, if practiced in pneumonia, should be resorted to, if at all, in the earlier stages of the disease. Rason (*Annals de Therapeutica*) treated 648 cases in the Hospital of Milan, by large doses of tartar emetic, of which 555 were cured, and 143 died, or 1 in $4\frac{1}{2}$ —which Rason contends is a result more favorable than can be obtained by blood-letting. M. Grisolle (*Traite Practique de la Pneumonie*) mentions one group of 50 cases, which were bled in the first stages of the disease only; and of these only 5 died, or 1 in 10. Those that died had been most freely bled, each having lost in successive bleedings about 4 lb. 4 oz. of blood. It may be further stated that these 50 cases were uncomplicated, and of the average age of 40 years. Of another group “of 182 cases, that were bled in the second stage, 32 died, or more than 1 in 6.” Of these it was also observed that those who died were those who had been most freely bled. These results were better than those of either M. Louis or Lænnec, both of whom bled more largely than M. Grisolle. In this country, it is doubtful whether the mortality in our hospitals has been so small as in those of Edinburgh and Paris. For want of proper statistics, however, it is impossible to speak with certainty. There are extant some statistics of treatment by diet, with the results of which the above figures may be compared, as throwing very much light on this important subject.

Dr. Balfour records the dietetic treatment of Skoda in the Charity Hospital of Vienna. In this treatment, if there was much pain, a little opium was given, and in the outset of the disease if there was much dyspnoea, venesection was practiced, and emetics given if the expectorations were a tough mucus.

And of 392 patients thus treated 54 died, or 1 in $7\frac{1}{4}$. Dr. Dietl treated 380 cases of primary pneumonia in the Charity Hospital of Vienna. Of these 85 were treated by venesection, and of which 17 died, showing a mortality of 1 in 5. 106 were treated by large doses of tartar emetic, of which 22 died, or 1 in $5\frac{1}{2}$, and 189 by *diet only*, of which only 14 died, or 1 in $13\frac{1}{2}$. Those treated by diet who died, had their cases complicated, while of the 85 who had been bled, and of whom 17 died, 7 had no complication of their cases. These figures show beyond question that blood-letting and the administration of tartar emetic do not either belong to a proper treatment of pure pneumonia.

Dr. J. Hughes Bennett found that in eight years he had treated 65 cases, of the average age of 31. Of these 65 cases, 62 were dismissed cured, and 3 died, showing a mortality of only 1 in $21\frac{2}{3}$. Dr. Bennett says, "The treatment I have pursued in pneumonia is founded on the pathological principles formerly given, viz.: never to attempt to cut short the disease, or to weaken the pulse and vital powers; but, on the contrary, to further the necessary changes which the exudation must undergo, in order to be fully excreted from the economy. To this end, during the period of febrile excitement, I content myself with giving salines in small doses, with a view of diminishing the viscosity of the blood. As soon as the pulse becomes soft, I order good beef tea and nutrients; and if there be weakness, from eight to ten ounces of wine daily. As the period of crisis approaches, I give a diuretic, generally consisting of 3ss. of nitric ether, sometimes combined with mxx. of colchicum wine, three times daily, to favor the excretion of urates. But if crisis occurs by sweat or stool, I take care not to check it in any way."—*Ed. Med. Jour. No. XXI., p. 792.*

Here, then, we are plainly told by one of the most eminent practitioners in Great Britain, that pneumonias in the young and vigorous almost always get well, if let alone. While the reverse of this is true under the antiphlogistic treatment of the accepted standard books. The mortality under the new treatment has been reduced from 1 in 3 under the old, to 1 in $21\frac{2}{3}$ under the new. These facts are certainly palpable enough to cause the advocates of blood-letting to stop and calmly investigate the pathology under which such a practice has been tolerated. If the profession has been wrong in this case, how could it be right in others?

Of mercurials, Dr. Bennett thus speaks: "As to mercurials, the confident belief in their power of causing absorption of lymph, by operating on the blood, is not only opposed to sound theory, as formerly explained; but, like blood-letting, is not supported by that experience which has been so confidently appealed to in their favor." Is not the above declaration

identical with the position occupied by the eclectic branch of the medical profession in America? Yet, notwithstanding all the abuse heaped on us for our heresy, the great men of our profession are gradually coming to the same conclusions. Not only is this so in Europe, but it is so in this country. Every farmer's boy can tell you that the practice of physic is very much modified from what it was; that it is a fact of universal comment. Now, why is this so? Why does the profession not abstract blood, as it formerly did, for almost every complaint? Why are mercurials not given to the same extent that they formerly were? Is not the answer obvious? It depends on our advanced knowledge of pathology, and the consequent greater certainty in diagnosis.

In this, as in other medical matters, I desire to treat every physician, as well as his theory, with due respect; but I am not called on to hold my peace when I honestly believe there are errors to be exposed.

I shall now fairly and without prejudice enter on an examination of the essential nature of inflammation, and I hope that I may be able to illustrate the subject so as fairly to bring it to the perfect understanding of all intelligent physicians.

INFLAMMATION.

A correct knowledge of the phenomena, causes, laws, objects and results of inflammation lies at the very basis of a rational practice, either in medicine or surgery. Yet of this important chain of phenomena, which is termed inflammation, so very little is known that one would think the sentiments of Travers and Paget, of which the above is only a reflection, had never been heralded to the profession. The profession has relied upon John Hunter's explanation, without even asking itself if that explanation was consistent with the more exact science of pathological anatomy. The phenomena of inflammation have been misinterpreted; the productive causes have been overlooked; the laws by which it is governed have been unsought; the objects for its establishment have not been understood; and, consequently, the end to be attained by the results has been entirely misconstrued. Let us examine as many books as we please on the subject of inflammation, and if we have set out to earnestly investigate the subject, we shall be driven to the conclusions I have expressed in the preceding paragraph. I have read so many theories on the subject of inflammation, emanating from the most eminent men, who were so positive that they had each advanced the true theory, and that all others were, therefore, wrong; and I have found so much of the same confusion and indefinite expression, and

so much inconsistency, not only in those theories, but in regard to the simple laws of life—physiology—that it is with very great hesitancy I can bring myself to the task of attempting a rational theory of inflammation. It is true that I have these theories, facts and experiments before me. It is true that I have witnessed all the phenomena usually seen in a case of inflammation; that with my chemical tests I have investigated the character of the blood when it was present; that I have taken my microscope and examined the minute structure of parts in such a state; yet I do not feel that I should be justified in very readily advancing positive opinions in relation to inflammation. I know that many of the movements of the animal organism are so obscured that we are liable to very grave errors of observation, and, therefore, very apt to adopt hasty conclusions.

It is very possible that I may satisfy my readers, but I fear that I shall hardly satisfy myself as to the explanation I shall offer. Yet so important is the subject, so necessary is it to comprehend the main features of inflammation, that if I passed it lightly over, I should regard the basis of this work as most incomplete. Upon the old pathology of the books, the eclectic practice would prove vastly injurious in the treatment of inflammation; but our experience shows this not to be the case, and if, therefore, I may not be enabled to give a philosophical explanation for our practice, I shall have but little difficulty in convincing the profession that the old theories are wrong. Our treatment of inflammation is diametrically opposite to that of the old system, or that which is based on the old theories; and as it is successful, evidently our theory is right, and will so prove to be, even though I may not clearly express that theory. I am perfectly aware that others in the branch of the profession to which I belong, have written on this subject; that they enjoyed fine opportunities for observation, and yet when I see how little has been done by our best writers, I cannot but feel that much remains to be done—that, in fact, there is no alternative but to go back to the diseased human body, studying its morbid anatomy, and closely watching the tissues in all their changes.

This I may not do to my own satisfaction, but at least I will give the results of my own experience, and the consequent conclusions. I make no pretensions to infallibility, and may err in my judgment, just as others do, but I will at least take in great kindness an exposure of my errors, because it is my earnest desire to arrive at the truth, and to communicate it for the common good of my fellow-men. It does seem to me that most writers on the theory and practice of medicine have failed to recognize the absolute necessity of basing their practice on clearly recognized and demonstrable pathological laws. And

in tracing these, we must not lose sight of physiology, or else we shall commit the grossest blunders. The study of every function, whatever its state, may be essentially necessary.

The eclectic branch of the profession has not been as well represented in its literature as we could have wished; and this is so for various reasons, chief among which has been the difficulty of time, as most of those capable of writing have been too much engaged in practice to have an opportunity to do themselves justice. It is not, therefore, strange that many disconnected essays on the various departments of the practice should have appeared, when no systematic effort has been made to exhibit the rational basis of the school. But the weight and importance of this branch of the profession is now such that the demand for such a philosophical exposition of eclecticism is imperative, and with the view of satisfying this demand, I am engaged on this work, of which I trust the profession will have no occasion to complain.

With the aim of presenting the sum and substance of what is really known in reference to the essential nature of inflammation, I shall commence at the very foundation of every pathological state—the physiology of the animal frame; for in studying the departures from physiological action, we are necessarily led to an appreciation of the pathological states. Before proceeding further, however, I may be excused for diverging a little, so as to examine very briefly the value of names as frequently applied to states in medical science. By a dogged adherence to names, which often have no real application, or which convey a wrong idea of the nature of an affection or state, our profession has stood greatly in its own light. Thus we have inflammation, from *inflammo*, to burn, a name which, while significant enough as indicating the increased heat or oxidation of the part, has been made to propagate an impression that the combination of phenomena which we term inflammation, depends upon the presence of a single morbid agent or phenomenon, whose nature was best represented by the phrase, *I burn*. Names are frequently so very arbitrary as to have no connection whatever; and this is so in medicine, much too often for the good of society. There is scarcely a treatise on medicine in which a large amount of space is not occupied in solving the value of names. For my own part, I have determined to use as plain and direct language as I can, and when I have to use imperfect and inappropriate designations, I note and explain them.

I know it to be a thankless task to undertake the innovation of new words, even though the circumstances may require it. It is a matter of great difficulty to change long established usage, no odds what its nature may be. It is almost as difficult to change the ancient faith of a people in religious matters, as to

introduce a new technical language where one had been previously received, as we may observe in the case of the brain and heart. The ancients referred all the passions and sentiments to the heart, but the moderns have shown the brain to be the instrument of intellection, yet we still hear of the bad and good heart, the warm affections of the heart, etc. ; and yet the very persons who thus cling to the old technical language will admit that the passions and sentiments belong to the brain. Rokitsansky's idea that the naming of disease is well enough, if in using those names we simply dismiss the theories which first led to their adoption, is sufficient as far as it goes ; but he might have added, this is, however, rarely the case. Our common experience tells us that not unfrequently we hear the advocate for one of those old and exploded theories, appealing to the etymology of the characteristic word in proof of the correctness of his position. The various medical associations could not do a better deed than to correct the errors of names. As a general thing, the names should be anglicised as far as possible, and, when this proves to be difficult, then the most expressive French, German, or Latin words should be used.

It is almost impossible to define inflammation so as to make such a definition satisfactory. Hence, if I attempt such a definition, it shall be after I have clearly set forth the causes and processes incident thereto. I am very unwilling to assume a position, and then bend the facts of science to its accommodation, for I have seen so much of the disastrous results of this course, that I would not willingly lay myself liable to the criticisms of the profession, at least when I myself should feel the justness of those criticisms. In studying the phenomena of inflammation, we must proceed with extreme caution, and without reference to the theories in vogue at present, or we shall assuredly not give to each phenomenon its true value. We are to go back to the body, and by the aids furnished by the researches of morbid anatomists, endeavor to learn not only what are the actual phenomena of inflammation, but the causes of those phenomena. When we begin our work in this way it is highly probable that we shall obtain the solution of the various problems of inflammation on a correct basis. But even here we must exercise the greatest care, for evidently the inflammation will be modified as it occurs in different tissues, and will be more or less marked as the causes have been more or less severe. Supposing that we have commenced our observation immediately on the establishment of the cause, and the exciting cause and tissue to be of average conditions, we shall observe the following well marked phenomena :

1. A simultaneous contraction of the capillaries, and a quickening of the stream of blood passing through them.

But unless the causes are only of a moderate nature, we shall not always observe these phenomena.

2. This contraction of the capillaries is, sooner or later, and possibly as a reaction of the contraction, followed by a dilatation of the capillaries. When the stimulus has been potent in the first instance, we may not observe the contractions at all; the first visible phenomena being dilatation. This has led to some errors in regard to the phenomena of inflammation in the human species, because the stimulus is not generally so powerful as to produce the dilatation in such proximity to the contraction as not to be noticed, while in artificial experiments on animals we are very apt to notice the dilatation first and alone, and if we do not reason well, may be led to suppose this is always the case, irrespective of the productive causes. This dilatation of the capillaries is always seen, both in the living and dead subject; nor shall we fail to notice the simultaneous loading of the capillaries with an increased volume of blood. It is this injection or engorgement of the capillaries with an increased amount of blood, that produces the redness of inflamed surfaces. This enlarged column moves slower and slower as the vessel becomes more and more engorged, until, as Rokitsansky well remarks, "it merges in an oscillating movement of the blood in the capillaries." The column moves onward and then backward, as a tidal stream, in which the onward movement slightly predominates.

3. The blood cells now begin to agglutinate and to hang together, as if by some peculiar attraction. The plasma itself does not seem to have undergone much change, though evidently it has been considerably modified. The cells have and are giving up all their oxygen; an undue amount of carbonic acid is being disengaged, and the cells themselves are being speedily oxydized.

4. Stasis, or retardation of the blood in the capillaries, is next noticed. Cell on cell is being crowded into the place occupied by the plasma; the cells become more flattened, and of a deeper tinge, owing, perhaps, to their greater number and closer adherence; they stick closely to each other, and to the walls of the blood-vessels, forming almost a common mass, with an occasional interspace filled with nucleated or colorless corpuscles, which have enormously increased their proportion. There is also noticed now, two remarkable phenomena, of which we must not lose sight. I refer to the laceration of the capillaries and the extravasation of the blood into the free spaces of the tissue so inflamed. As a result of this, where the textures are of a delicate nature, there is often considerable hemorrhage, constituting what has been termed capillary apoplexy, and the exosmosis or transudation of the serum through the thinned walls of the blood-vessels into the parenchyma,

infusing a dew as it were, through the parts, which collects in reservoirs, as in the case of blisters, burns, erysipelas, etc. The serum thus thrown out is that which was formerly in the blood, and resembles it, except that it has given up a part of its albumen. This collection of exosmosed serum may advance to external exudation, when it appears as a moist dew on the surface, from which it is rapidly evaporated.

5. Genuine effusion now follows. By this I mean the exudation of true blood plasma, holding in solution fibrin, albumen, and various salts. This is poured into the interstices of the parenchyma, and may retain its fluidity or be more or less solid, especially if there has been much surface evaporation.

So much, then, for the attending phenomena of inflammation. This is the climax, for with exudation the true inflammatory process is ended, and the next series of phenomena is to get back to the normal point where all these phenomena commenced. From the capillaries, thus far, exosmosis has been the order. This is now changed to endosmosis of the serous portion of the exuded matter. Presently we observe that the capillaries begin to contain more fluid; that the blood cells begin to float more easily, and finally begin to separate. They become more spherical, of a lighter color, and finally begin to move around through the capillaries in the natural way.

These, then, are the prominent phenomena of a simple case of inflammation, about which more books have been written than I even feel inclined to enumerate. It will be seen that there is a vast field for speculation and theory, and it has been well occupied, most assuredly. For my part, I shall pay but little attention to these speculative theories, and shall try to confine myself as closely as possible to what seems to me the pathological explanation of these phenomena, always relying on the researches of the morbid anatomists for light on this most difficult subject.

1. The contraction of the capillaries and the attendant acceleration of the blood stream is a vital phenomenon, and it is to be remarked that this contraction is independent of any and all shrinking up of the surrounding tissues, as we observe when the body or part has been subjected to a very low temperature.

2. The dilatation is always constant, and, as before stated, often succeeds the contraction so very rapidly that it is regarded as the original phenomenon. This dilatation is not the result of relaxation and exhaustion merely; it is the result of a series of circumstances: 1st, The impacting of the blood cells in the capillaries; 2d, The retardation of the blood in its course; 3d, The elasticity of the capillary walls, etc.

With these two phenomena—the contraction and quickening; and dilatation and retardation of the vessels and blood—most writers on inflammation have dealt, but manifestly such writers have not gone far enough; they have not watched the phenomena in their varied changes.

To explain them, a very noted writer set up a theory by which it is contended that the contraction is owing to an attraction of the diseased parenchyma for the blood, producing exosmosis. This theory regards the retardation of the blood in the capillaries as the primary phenomenon, and the dilatation as a subsequent and consequential affair. The error of this is so palpable that I need not stop to refute it. But how is this attraction exerted? This theory ascribes to the nerves the power of generating it; it also uses the same nerves to account for the increased flow of blood to the diseased part. The dilatation of the blood-vessels is accounted for in the homœopathic theory by claiming that they do so dilate in consequence of a paralysis of the nervous fibrils. That the nerves have much to do with these phenomena we shall hereafter see, but the toning up or depression of the walls of the blood-vessels by their supplying nerves does not explain all the distinctive phenomena of inflammation.

3. The coherence of the blood disks is owing to the same causes which produce dilatation—exosmosis of serum and mechanical contact in the confined walls of the vessels, as also the altered state of the disks and remaining plasma.

4. Stasis is dependent on several causes. Among them is the coherence of the blood cells, and their impaction in the vessels, and the exosmosis of the blood plasma. Again, it is also promoted by the thickening of the plasma, the increase of fibrin and albumen, and the transudation of serum through the walls of the blood-vessels; and, what is more important than all these, the accumulation of nuclei and cell-formation and blood corpuscles, their adherence, and the presence of the transparent, fibrinous coagula, collectively developed. These phenomena are of the greatest importance, for they clearly illustrate the stasis itself, and at the same time reveal the formation or plastic processes in which the blood is engaged at this stage. It is here that we observe the difference between simple exudation, and the inflammatory process. It is at this point that we observe the new formation elements arising out of the blood, as also the changes which the blood itself is undergoing. The blood as now seen is of a dark tile color, with floating particles of cruor, twining with the new formation elements, and coagula, in which we observe a curious admixture of the old flattened blood disks, the new formation elements, etc.

5. In the dilatation or enlargement of the blood-vessels,

there is a consequent thinning, so we should have to show from whence this additional matter of enlargement was derived. This thinning of the walls of the vessels lies at the basis of the phenomenon of exudation. There is also established and maintained during this phenomenon all the peculiarities of endosmosis and exosmosis—simple, natural laws alone being employed in the production of the phenomenon.

A state of congestion surrounds the seat of the inflammation, and where this is present, there also occurs an effusion of serum. The congestion is less marked as we leave the seat of inflammation, and also the serum becomes poorer in the new formation materials. It is this that gives us the phenomena of œdema accompanying inflammation.

We come now to the more visible phenomena of pain, redness, heat and swelling. These are always of great importance in diagnosis, because they constitute the visible signs which are to direct the physician.

The *pain* incidental to inflammation is determined by a variety of circumstances, most of which I shall briefly notice, and, as I hope, in a way to render the subject intelligible.

1. It is caused either by the action of the exciting cause of the inflammation on the peripheral nerves, or else by a reflection of these causes from the central organs. But in the language of Rokitansky, "in no instance is it determined by the inflammation itself;" and yet it is almost invariably referred to the inflammation. This is one of the many errors which are to be erased from the minds of physicians before we can expect that absolute correctness of practice based on sound pathological principles, for which eclectic physicians have long been contending.

2. Inflammation is determined by the pressure incidental to the distension, dilatation and engorgement of the blood-vessels, and the distension of the intercellular spaces of the parenchyma, in consequence of the attendant effusion—all of which physically or mechanically affect the adjacent nerves. This is true inflammatory pain, and is referable to the abnormal condition of the parts involved.

3. In every inflamed part, as we have seen, there has been an increased oxydation, and therefore an increased evolution of carbonic acid. Now there can be no such oxydation and evolution of carbonic acid, except there be also a corresponding elevation of temperature. In the presence of such an increased temperature, the nerves are more sensitive, and therefore a part of this pain, or a certain degree of it, is determined by an increase of temperature of the inflamed part.

The *redness* is determined by the undue injection of the capillaries with blood cells or disks. It has been already shown that one of the principal phenomena of inflammation

was the impacting of an excessive number of red blood disks in the vessels, and it is the increased number of these which determine the redness of inflammation. It was formerly thought that in every well marked case of inflammation, to accommodate the increased flow of blood to a part so inflamed, a large number of new but temporary vessels were formed; and this increase of vessels was supposed to account for the increased redness. As it is now known that no such new formation of vessels does occur, therefore this was an error which in our day and time must be discarded. It may with some show of justness be claimed that a part of the increased redness is owing to the exosmosis of blood during the stages of congestion and stasis. In some kinds of inflammation the blood pigment is dissolved, and exosmosed into the tissues. This would necessarily produce a degree of redness which has been termed the *redness of imbibition*. The redness will be somewhat altered in form as the capillaries are distributed in different ways, but this circumstance is by no means important, since in the most delicate tissues we observe a uniform red tint, so complete is the injection.

The intensity of the redness is also dependent on the character of the inflamed tissue—that is, whether it is more or less vascular—and the intensity of the congestion. Again, the intensity of the redness of inflammation depends largely on the constitution of the blood itself. This we see illustrated in syphilitic and in typhus inflammation, the color being coppery in the one and violet in the other. I have already shown the cause of the increased temperature of an inflamed part, but I may in addition remark that the formative processes which are going on are themselves the result of chemical changes, and in the production of these changes there is always an increase of temperature; for many of them cannot be accomplished except in the presence of an increased temperature.

The *swelling* is easily explainable, since it cannot but be apparent that it depends on the dilatation and engorgement of the capillaries; upon the exudation of blood serum and plasma in the parenchyma, and upon extravasation. Upon this exudation and extravasation depends that peculiar softness and yet even solidity of inflamed parts.

So much, then, for a general description of the phenomena of inflammation. Upon a correct understanding of these, the whole practice of medicine depends for its rationality.

Were I to close this subject at this point, I should have greatly failed to do full justice to the subject of inflammation. Being aware of this, I shall proceed in that inductive course which I deem best to lead the mind on, step by step, until all the mysteries of this pathological state are clearly explained.

It will be understood that I must now treat of the varieties

of inflammation, the products of inflammation, and finally the issues of inflammation. This labor I deem absolutely essential to explain much that has been said, and certainly much that will occur to the physician in practice. Of these various subjects I may be compelled to make divisions and subdivisions, but I trust that in the general summing up every fact will be fully explained and understood. Again, when treating of special disease, I shall be compelled to retrace much of the field of labor in which I am now engaged, and specifically consider the phenomena of each and every case. It is at those places that my readers should expect of me a full exposition of every minutia. Did I not know the difficulties which I have undertaken to remove, I should perhaps be willing to enter into an unsystematic examination of this subject in all its details; but I believe such a course would not only confuse the minds of my readers, but it would lay me open to the promulgation of many an error, which by system and a close study of every part, I may be able to detect and avoid.

The readers of this work will perceive that I have undertaken the exposition of the errors of the great mass of the profession, and while that would, in my judgment, be an easy task, I do not think the work would be a commendable one, unless I can establish a surer and more scientific basis for the practice of medicine. That such a change is demanded is, I think, quite apparent when we consider the want of success in general practice. That there is a prospect for such a change, is apparent from the rapid advances made by the sciences of physiology and pathology. As we understand these subjects better, we necessarily modify our practice, as we then more clearly see and comprehend the phenomena of physiological life; and consequently we are more easily enabled to understand the causes which are operative in the production of morbid states or pathological phenomena.

A discovery in physiology is the signal for a revolution in medicine, and the same may be said of pathology. By cultivating those two branches of our science, by collecting the observations of all, and accurately weighing them, we shall eventually obtain positive theories on which to base a practice of medicine. It will then remain for us to correct our *materia medica*, and then take our stand boldly as the true conservatives of the public health.

It may be said, we shall never live to see such a triumph of medical science. Well, we may not. We did not witness the laying of the corner-stone of the medical fabric by Hippocrates, and we have no right to refuse to labor for its advancement because, perhaps, we may not witness the putting on the cap-stone of the monument of medical science. It is the duty of each man who enters this profession, to cultivate and adorn

the science in his own day and generation, and then bequeath his work to those who are to follow him. For my own part, I am satisfied to do a little good; to know at last that I have done only a little in polishing up and placing one stone in the walls of this vast medical tower, which has already been in process of erection four or five thousand years.

CHAPTER IV.

CLASSIFICATION OF THE VARIOUS FORMS OF DISEASE.

ALTHOUGH the forms or manifestations of disease are too numerous to admit of computation, yet all may be arranged under two heads or classes, the febrile and non-febrile.

There is no form of disease that will not readily fall into one class or the other, and between those of the two classes respectively, there is no repugnance of character. It is true, that sometimes a form which is essentially, in its general character, non-febrile, may become febrile, but this is a mere circumstance, and as an objection, its magnitude is not greater than will occur in any other division that may be attempted; at all events, it suits the circumstances of our present purpose better than any other that has been suggested to us.

As it is almost impossible for any physician, in the beginning of any form of disease, to entertain a doubt as to which class it belongs, so this division keeps constantly before his mind the leading idea of his duty. The limits, furthermore, that bound the two classes, are, usually so clearly defined, that the pulse alone will generally furnish a sufficient diagnosis.

We shall begin with the febrile class, or that which is distinguished by a special manifestation of the vital force—such as will frequently, without the aid of medicine, restore the patient to health. There is yet another reason why we should begin with the febrile class—it is more simple, because it is more easy to reduce force than increase it.

PYREXIA OR FEBRILE FORMS OF DISEASE IN GENERAL.

The opinions upon the nature and character of fever are about as various and as numerous as have been their many

writers, and if we commence their investigation with Hippocrates, who lived 361 years before the Christian era, and who was probably the first systematic writer on the subject, we shall find them, as we travel down to the present time, to depart more and more from our conceptions of the truth.

He regarded the human system as being under the direction of a conservative, and in some sense, intelligent principle, which he denominated *nature*. Under such a conviction he must have believed that pathological manifestations were governed by laws as fixed and as determinate as those of gravitation—that all pathological action is just as legitimate and as normal, under the circumstances, as any purely physiological one. This inference from the preceding premise, is completely sustained by his opinion of disease, as set forth by the *Edinburgh Practice*, Vol. I, page 6; viz: “He imagined disease to be only a disturbance of the animal economy, with which nature was perpetually at variance, and using her utmost endeavors to expel the offending cause.”

In this simple, consistent and beautiful faith, he appears to have been well grounded; for Dr. Thatcher, page 4, says, that “he studied and copied nature with the greatest care and assiduity, as the only sure basis of medical science; and so extensive was his knowledge, and so accurate were his observations, that he has been constantly held in veneration through succeeding generations.” From existing indications, it would seem, however, that “succeeding generations” venerated him, not because of his talents or the accuracy of his observations and discriminations, but as they generally do any old antiquated ruin—a proper veneration would have dictated a closer conformity to the immutable and salutary laws which he was the first to discover and proclaim.

The animal system, as a machine or organization, does not act intelligently, but in accordance with the laws of that pre-existing intelligence which designed it. Between this opinion and that of Hippocrates, there is not, practically, a shade of difference, and it is to be presumed that no one can be found who is so much of an accidentalist, as to deny that the human organization, either in the abstract, or in its relations with the external world, does indicate wisdom or intelligence in its design. If this conclusion be admitted as correct, then it follows that Hippocrates was in the possession of a great discovery, when he remarked that “Nature cures disease”—that is, disease is removed from the system by the agency of those laws of the organization which exist and act in conformity with a wise or an intelligent design. It follows again, with equal conclusiveness, that man has no power to cure disease—he can only aid “*nature*,” by acting in conformity with her laws, and when he acts otherwise, he is sure to defeat her in-

tentions. This conviction should be ever present in the mind of every physician, when at the bedside of the sick.

We have examined very many of our standard and most distinguished authors on the pathology of fever, but our taste, judgment, prejudice, partiality, or whatever else it may be, is too fastidious to adopt or indorse any opinion we have found, except that of John Hunter in the abstract, and that of Tissot, in connection with its treatment. The former says, that "fever, in all cases or of all kinds, is a disturbed action, like inflammation itself." And "inflammation," he says, "is not to be considered a disease, but as a salutary operation either to some violence or some disease." He again remarks, that "pure inflammation is rather an effort of nature than a disease." Then, according to Hunter, we may say that "pure" fever "is rather an effort than a disease;" and, consequently, as fever is but action, all fever must be pure.

The latter author, Tissot, says, that "a fever, therefore, that has a quicker pulse than natural, and an increased degree of heat, is always salutary with respect to the morbid cause; for the effect of the morbid cause is excellently fitted to remove the cause itself.*" Hence a fever is justly defined by the celebrated Sydenham, to be a "vigorous effort of nature to throw off the morbid matter, which is extremely inimicable to the human body, and thus recover the patient."

"A fever, therefore, which is not too violent, but suited to the morbid causes, ought not to be extinguished, if it could be done, but rather kept up to promote their expulsion."

It would seem that there is no war between the Eclectics and Old School Allopathists of former days—the former are actively engaged in repairing, enlarging and elevating the original edifice; while the modern Allopathists are not only hostile to those who abide in the faith, but have disowned their parentage, as will presently appear.

Thatcher, Gregory, Clutterbuck, Rush, Watson, Eberle, Wood, and Cheyne, regard fever as a disease. Thatcher calls it "*the disease*," and the others though not so brief, agree with him. All of them have failed to find a definition of fever; and why? Simply because they have regarded it, as they have inflammation, to be a disease, and therefore like the latter, in the language of Dr. John Thompson, is, in every case, either an "*effect, a concomitant, a cause, a symptom, or a consequence*." We should indeed think it difficult to define such a proteus.

Under an abiding conviction that no one can be a good or a safe practitioner who is directed by false principles, it be-

*This is to be understood according to the definition of Sydenham.

comes our duty to dwell on this subject to such an extent as we may deem necessary to an exposure of the prevailing errors, and the defense of those which our convictions of truth compel us to adopt.

As it has not been our good fortune to find such a definition or conception of fever as we can adopt, we are called upon to add one more to the long catalogue with which our medical libraries are now burdened. We would, therefore, define fever to be that amount of vital action, *cæteris paribus*, which any given, but general, obstruction to the vital functions can produce. It follows, consequently, that that amount of obstruction which would produce but a slight fever in one of strong vital force, might produce death in another of extremely feeble vital force. Consequently there can be no grade of fever which cannot be thus readily understood, so far as regards its modes of manifestation.

As to the location of fever and its proximate cause, or as we would say, as to the nature of the disease that produces fever, there are many and widely different opinions.

Dr. Cheyne, Cyc. Prac. Med., in opposition to some of the opinions we have quoted says, that "we find the early medical writers entertaining the idea that the system waged war against something noxious within itself, and that in the attempt to expel the offending agent, a violent commotion was excited." By this plausible theory, the duties of the physician were restricted to assisting nature in her efforts to get rid of what was deemed injurious to the welfare of the body; in fact, fever was imagined to be a natural and salutary process, indispensably necessary to throw off whatever was noxious, whether generated within the body or introduced by external causes."

This extract shows very clearly that the author considers fever to be a disease, and that the physician has something more to do than to assist nature. This is particularly that feature of the Old School practice which we lament—they take it upon themselves to remove disease, and so far from helping nature, they throw about one-half of her out-door in the form of blood, and the remaining half they poison to death; but we, more humble in our pretensions, will feel entirely satisfied with our efforts, when we shall become qualified to render the necessary aid. Beyond this humble position we have no ambition.

We are very much mistaken, however, if the author has not shown himself to be exceedingly inconsistent. In the same paper he remarks: "It is more than probable that in what is usually called *idiopathic fever*, there is alteration either of the solids or fluids, although its precise locality cannot in every case be detected; but without *disease* in either the one system

or the other, we maintain that fever cannot exist." This is precisely our position—when there is no disease in the system there can be no demand for fever. In the previous extract, the inference is clear that fever is disease, and that all those are in great error who regard it as an effort of the vital force to cast out disease, and now he says, that unless there is disease fever "cannot exist." Fever, then, is an effect, and not a disease, nor is it the cause of the disease. Then it follows, that fever is a friend of the system—an effort of the vital force, or rather an accumulation of it for the purpose of removing disease, or else it is an enemy of the system, and if so, then it is disease, and as such, it is superadded to the disease that caused it. Verily, if this is the case, we need not to marvel at the difficulty of comprehending it. No matter where we turn, we find nothing but confusion as to what fever is, and when we inquire as to its proximate origin, we obtain nothing more satisfactory.

Many have, some yet do contend, that the cause of fever is to be sought in the fluids, while others are equally sure that it is in the solids. Some have referred it to the nervous system, and some again have attributed it to some mysterious or occult organic inflammation. Fever has been hunted for more than two thousand years, and yet its hiding places are as little known as when the search commenced.

Dr. Stevens is of opinion that a morbid condition of the blood is the first link in the chain of morbid phenomena that causes fever—that the ærial or miasmatic poison reaches the blood through the respiratory function. He seems to have founded this opinion upon such facts as this: he states that on opening the heart in fatal cases of yellow fever, he found, instead of blood, a dissolved fluid nearly as thin as water and black as ink. In both sides of the heart the fluid was equally black, and throughout the vascular system all distinction between venous and arterial blood was completely lost.

If this supposed ærial poison has the power to do all this mischief by acting directly on the blood, it must be considered as exceedingly singular that human genius can devise no means to detect its presence in the atmosphere, or in the blood, or under any other possible circumstance. To the doctrines of fever we know of but one parallel, and that is geology—with regard to both, many facts have been accumulated, but the advocates of both seem to have banished common sense from the field of their investigations, and given themselves up to the most extravagant speculation concerning them—summoning to their aid the most occult, hidden and extraordinary causes—such as would elucidate nothing if admitted.

As to the remote cause of fever, there is a pretty general unanimity of opinion, it is an ærial poison, miasm, malaria,

an ignis fatuus, a something, a nothing, and yet it produces the most fatal types of fever. It is not our purpose to enter further into the discussion of this subject, or to do more than simply to record our own convictions of it. We have observed enough to satisfy us that heat and moisture, with the plus and minus electricity they occasion, and the variously modified modes of life among the people, are adequate to all the febrile results we have noticed in connection with them.

We cannot believe that any one will venture to contend that fever ever did take place under a proper performance of the depurating function. Inflammation results from local obstructions or lesions. Now suppose such obstruction or lesion shall be general—universal in the system, what else could or should be expected than such a reaction as constitutes fever? Every part and parcel of the solids is so over excited, or under excited, that no one part, however small or comparatively unimportant, is able to eliminate its metamorphosed portions—the absorbents are as it were dead. The glandular structures, too, are in a similar condition, and hence all the impurities of the venous blood, because of defective depuration, becomes mixed or blended with the arterial, and this morbidly excites every part and fever results.

It is well known that a hot, dry season is measurably exempt from febrile epidemics, but if rain be added to this heat, then they appear.* We are told that it is because the two combined promote vegetable decomposition. If this explanation be adopted, how shall we account for some epidemics in sandy deserts, far removed from any source of malaria? It is well known that children and negroes are greatly exempt from the malarious fevers of the South. If then, these fevers are produced by a poison, how does it happen that they are exempted? If our position be correct, this fact is easily explained. The black surface of the negro radiates not only a large portion of his animal heat, but also a large portion of that solar heat that falls upon him; and another large portion is removed by the evaporation of his perspirable matter. With these advantages his skin maintains its integrity, and as no morbid sympathy is engendered between it and the liver, the kidneys and other glandular structures, a proper depuration of the system does not become suspended; and as to children, they are confined mostly to the house. But they have another means of protection—it is that thick coat of adeps which is common to their age, and to some forms of these epidemics such men are equally invulnerable.

* This is not the fact, with reference to the yellow fever of New Orleans: a hot, dry season is the most productive of it; we admit the statement, however, as regards intermittents.

No medical philosopher, we should think, could have labored for many weeks, contrary to his custom, in a hot sun, with the other epidemic requisite, and not comprehend how it was he contracted fever. He exhausted his skin—many internal organs sympathized with it, particularly the liver and mucous membrane of the bowels, and thus, without the prevalence of moisture, he may be seized with dysentery—but with the moisture he will have some form of fever—his skin is too feeble to contend with a greatly reduced electric condition of the atmosphere, constriction of it ensues, then comes a chill, and then a reaction which is denominated fever. As the obstruction is universal, the result is fever, and cannot be inflammation, and therefore it appears about as consistent to talk about a general or a universal congestion as about an inflammatory fever.

These views of fever strike us as being the legitimate results of common sense and common observation, and do, with clearness and force, point out the necessary indications of treatment. With a normal depuration, fever cannot obtain, and without it, it cannot be cured, but it may be replaced by a mercurial disease, or some other, or the patient may be killed, or suffered to die for the want of an adequate treatment.

We are aware that very many apparently conclusive facts can be brought to bear in favor of malarious poison, but let it be remembered that laws or rules have no exceptions, and therefore the production of a single epidemic, of the same kind, under circumstances that positively contra-indicate the existence of such a poison, is enough to destroy the doctrine—one affirmative fact is worth any number of negative ones.

Our observation proves that where a person has a low caloric and electric condition, the patient generally possessed a comparatively feeble vital force, or Professor Hall's high dynamis, and therefore could not bear a greater reduction of these stimuli. In the cases of continued or remittent (synocha) fever, to be confined to constitutions of high vital force, or according to Professor Hall's *Zoonomia*, high stimulus; and therefore could not bear more, as the high caloric and electric atmosphere.

With reference to fever in general, we remark that it does not matter what may be the number or the variety of the forms under which it may appear, it is essentially a unit—it has an increased action of all the fibrous tissues for the removal of disease or obstruction; nevertheless, for the convenience of study, its forms may be divided, and with much propriety, into two orders; first, those which result from general obstruction and therefore attended with fever; and secondly, those that result from obstruction, and consequently attended with inflammation.

We have too much to learn about the atmosphere, as regards caloric, electricity and humidity, to conclude, when it is fatal to animal life, that there is in it some foreign poison. The conclusion in this instance has been as much too hastily drawn as that of the geologists in referring mineral coal to a vegetable origin, upon the flimsy evidence that the coal shales contain vegetable impressions. Such hasty generalization has, and now is doing much to retard the advancement of science. Of this, we have one flagrant example: the whole medical world has admitted that some forms of disease are hereditary, and this general admission has closed the door to any useful investigation on the subject—and yet the character of the error has but to be named to be clearly perceived.

We have not dwelt upon this subject, to the least extent, with any expectation or even a desire to convert any one to our convictions of the truth, but for the purpose of awaking fresh observation—for the purpose of breaking up or disturbing that settled conviction of the truth of a conclusion which may possibly be false.

CONTINUED FEVER AND PERIODIC FEVER.

Before proceeding to the consideration of these genera, it may be profitable to dwell a while on the phenomena that usually constitute the course, type and stages of fever. The course of a fever is determined by its phenomena being continued, remitting or intermitting, and that series of phenomena which characterize this course may be usefully divided into the following periods or stages: 1, the forming; 2, the cold; 3, the hot; 4, the sweating, and 5, the final. There are so many interesting and useful phenomena in the first, that we have preferred to embrace it, though contrary to the custom of many. As the final period, that of convalescence or dissolution, is of equal interest with the first, we have, in company with some authors, thought it best to include it also.

It should be remembered, however, that we are not to expect to find all of these stages clearly defined in any one of the febrile forms, except the intermitting; nevertheless, periodicity characterizes, more or less, every form of disease; but this law is not particularly conspicuous in the violent and continuous forms of fever, and yet it but rarely happens that the primary or oppressive, the febrile or exciting, and the declining stages of all forms of fever are not sufficiently marked to be clearly observed.

1. *Period of Formation.*—This is, in reality the period in which the vital forces are preparing for the removal of existing disease, or obstruction to the normal manifestation of the physiological functions, it is that period which intervenes between the first morbid sensations, and the introduction of the

febrile phenomena. Its duration varies exceedingly, and this depends more upon the resisting character of the invaded constitution than upon other contingencies. In the highly vital, this period is occasionally so short as not to be observed—the febrile attack is absolutely one of surprise and astonishment. In such constitutions the faculty of animal sensibility is so strong, and therefore vigilant, that upon the slightest invasion, the other vital powers are immediately summoned to resistance.

In constitutions of a contrary character, this period may be greatly prolonged. The obstruction is permitted to increase and spread, until at length the physiological functions become very much arrested—too much so for a normal continuance of life, and then a struggle ensues which is indicated by a rigor or a chill, which is succeeded by fever.

The first indication of this prodromal stage is an indisposition to attend to business—a feeling of weariness, an indefinable anxiety—the appetite still being good. Then succeeds a sense of lassitude; disturbed sleep; loss of appetite; yawning and stretching; wandering pains in the limbs and back, and unpleasant sensations in the stomach; harsh and dry skin; irregularity of the bowels, but most generally constipated; some headache; flatulence; nausea; the drying up of old sores; giddiness; tremors of the extremities; slight creeping sensations of cold, and an indisposition to be satisfied with the usual modes of existence. In reviewing these symptoms, as a group, we are bound to perceive that the nervous system has become greatly impaired, and that its susceptibility to normal impressions has become blunted.

2. *The period of rigor or chill.*—The first indication of the approach of this stage is a sensation of chilliness which may be confined to the loins, or to one or more of the extremities, or to the spine, while the other portions of the body feel to be comfortably warm. This state of the system gradually passes into one of tremor, which first becomes manifest in the inferior jaw, and thence extending to the entire muscular system. In the firmer class of constitutions it but rarely exceeds a chill and slight tremor; but in less resisting constitutions it is sometimes very severe—so much so that it has, according to authorities, produced convulsions.

The patient has not only a sensation of cold, but his skin does become cold, and Dr. Wilson Philip says that its temperature has been known to fall as low as 70° Fahrenheit. This condition of the animal temperature becomes indicated by the paleness of the lips, nails, fingers and toes—sometimes they become of a purple or livid color. The skin on most parts of the body becomes greatly constricted and drawn into little elevations or papules, like those of the feathered tribe

when the feathers are removed. The pulse during this stage, is small and quick, and frequently irregular; the breathing, like the pulse, is hurried and labors under a feeling of constriction in the præcordia.

During this stage, the patient makes no demand for food, but his thirst is sometimes very considerable; the secretions are not only in a great measure suspended, but much deranged—the saliva is scanty and viscid; in some instances much gastric derangement obtains, as frequent retching and vomiting; and occasionally this irritation is extended to the bowels and liver, which is attended with a purging of bilious matter. These symptoms, however, are mostly confined to southern latitudes, but in all latitudes this stage is so mild that the patient himself does not become conscious of it—it is indicated only by some paleness of the lips and a little coolness of the hands and feet.

The length of time occupied by this stage is very variable—continuing from a few minutes to four or five hours, but one hour and a half may be assumed as the average duration. It may be valuable to remark, however, that this period is generally more protracted in southern than temperate latitudes; but in all latitudes it may be well to add, that the more this stage is contracted, the more protracted and severe will the hot one be. When we reflect that the chill is always in the inverse ratio to the vital or reactive force, the reason of this is readily understood; and hence it is that when the vital resistance or reaction cannot be effected, the patient dies in the chill.

When the reaction has been effected and completed, flushings of heat become blended with those of the chill; the patient breathes with more ease, and gradually removes, piece after piece, the bed-clothing off his body, and finally from his extremities. The hot stage has commenced.

3. *The period of increased temperature.*—When the temperature begins to rise out of the rigor, it is felt by the patient to be a great relief, but when it reaches its maximum intensity, he finds himself as uncomfortable as when at the other extreme. He first becomes sensible to the introduction of this stage about the face and eyes, and then the thorax, the abdomen, and finally the extremities. When the reaction is rendered difficult, sensations of heat and cold alternate, and frequently for some time, but finally, in such cases, the former obtains the ascendancy. The patient finally begins to feel hot, and the sensation is not delusive, for the condition of his surface produces the same sensation to the tact of others, and further, it becomes demonstrable by elevating the mercury to 105° , and in a few instances to 107° , when the bulb is placed under the tongue.

With this increase of temperature there is usually a very considerable acceleration of the circulation, but in this particular there is much variation. When the vital force and the obstruction are so related as to render the reaction easy, the pulse is full, strong, and quick, but without a remarkable frequency, not much more than is common to health. But when the condition is reversed, the pulse, in all respects, falls below the normal standard, except as to its frequency, being sometimes increased to 110, and from this to 160 beats in a minute; but while there is good velocity, there is indicated a great want of arterial power, for it is soft and easily compressed. If inflammation be located in any special part, it will be indicated by a tense, small, and contracted pulse.

The brain, as a matter of course, participates with the other parts of the system, and hence, in this stage, the senses, except those of smelling and tasting, become more acute, the brain is very frequently afflicted with pain, more particularly with those who possess dense and fibrous constitutions; and, moreover, if the fever run very high, delirium is a very general consequence. It is quite common in this stage for the patient to complain much of pain in the back, the extremities and other portions of the body; he is also troubled with a morbid vigilance—an indefinable uneasiness and restlessness.

The digestive system is not without equal manifestations of derangement. A loathing of food and warm drinks, with a desire of cold drinks, are peculiarly characteristic of this stage. Nausea and vomiting, constipation or diarrhea, but more frequently the former; a disagreeable taste in the mouth, a furred tongue with clean tip and edges, are also very common symptoms.

The secretions are generally very much arrested; the urine is scanty and high-colored; the skin, tongue and mouth are usually dry, and, in the most favorable cases, the bowels are dry and constipated.

The duration of this stage is as uncertain as that of the preceding; it sometimes ceases in a few hours, but in many instances it continues for days and even weeks. Finally, it should be remembered, that all of these symptoms become very much modified, in different cases, by circumstances incidental to the cause, the constitution or the peculiarities of the occasion.

4. *The period of perspiration.*—When the previous stage has become so far reduced in temperature as to admit of secretion, perspiration begins to break forth upon the superior portions of the body, and gradually extends over the whole surface. It is sometimes, as to quantity, quite moderate, but in many instances it becomes highly profuse. When this period has continued for some time, it subsides, leaving all the

functions very much in their normal condition, except that they are much weakened.

The preceding four periods succeed each other in regular order; nevertheless, they present much diversity of character in the different forms of the disease, as may be noticed when we come to treat of them individually.

5. *The period of conclusion.*—The disease for the removal of which the fever was instituted, was proximately occasioned by an insufficient depuration, so when this function begins to re-establish itself, and this stage begins in a favorable manner, the patient will recover, in the event of no unfavorable interruption. On the other hand, if the function of depuration be not re-established, the chemical agencies acquire an ascendancy over the vital, then this period is occupied by the progress of dissolution. Hence it is, that when the declination is favorable, its beginning is indicated frequently by the spontaneous occurrence of a profuse perspiration, urination, bilious or serous discharges from the bowels, which have been denominated *critical*.

We cannot avoid, in this place, to notice a remark of Dr. Wood, who says, "It has been conjectured that these evacuations were the means by which nature effected the cure of the complaint; and the idea at one time extensively prevailed that through their instrumentality offensive matter, which served to sustain the fever, was eliminated from the system. Without attempting here to confute this hypothetical notion, it is sufficient to say that the discharges are the necessary results of a certain grade of irritation in the organs, and if they serve to relieve the disorder in which they originate, it is only one of the numerous instances in which *nature has contrived* to make noxious influences subservient to their own removal."

If the bills of mortality could either speak or write, they would publish a most lamentable account of those who practice upon the doctrine here set forth by this distinguished professor. It is too glaringly absurd to require of us more than a very few common sense interrogatories and comments.

Will the doctor or any one else affirm that the patient would or could have recovered without the elimination of the morbid and offensive matter which is now under consideration? How did that "certain grade of irritation in the organs" happen? Was this matter elaborated by the irritation, or was it the cause of the irritation? If the "instances" of such eliminations are "numerous," should we attribute them to nature's bungling, or to a law of her settled policy? If nature very frequently bungles in this way, it would be a happy circumstance to society if the doctors would suffer her to manage all cases in her own way.

If secretion becomes arrested or deranged by miasmatic

poison (?), heat, cold, minus or plus electricity, or any other cause, and the matter which ought, normally, to have been eliminated, is not, will it not act upon the irritability of the system as foreign matter? And if the retention of such matter did not produce a "certain grade of irritation," and even fever, could the system be in a normal or physiological condition? Is there not, sometimes, too much carbon, urea, or lime, or phosphorus, in both the fluids and solids of the system? Has the system any other remedy for this, its condition, than secretion and depuration by the skin, kidneys, liver, the mucous membrane of the bowels, etc.? And are not those eliminations which take place in the crisis of fever, of this character? Do they not absolutely produce it, and thus terminate the fever by removing its cause?

In such instances *nature* has effected the very result which should be the object of the physician from the moment he visits a patient; indeed, he was called for the purpose of helping her to do in less time that which she may ultimately effect without help.

One of the most indubitable evidences of the commencement of this period—of the re-establishment of secretion—is the moistening and cleaning of the tongue. This cleaning process commences at the point and edges, and gradually progresses until the whole is clean and brought to a normal appearance. These changes in the tongue indicate, no doubt, that nature is "contriving" to do the same for the whole gastro-intestinal canal, in connection with a general secretory movement—the execution of the plan "which nature has contrived to" remove a "certain grade of irritation in the organs," instead of being the result of it.

With the indications of returning health, above named, may be expected a *pari passu* movement of all the secretions, and a corresponding return of the senses, appetites and propensities to their normal condition.

Before dismissing this subject, it is proper that we add a few remarks upon what is usually denominated the types of fever, and as Dr. Eberle's descriptions are better or more clear than those of most writers, we shall adopt his account of them.

He says, "The space of time occupied by one paroxysm of a fever and its succeeding intermission, or which intervenes between the regular periodical exacerbations of paroxysmal fever, is called the *revolution* of a fever. The revolutions of fevers are various in point of duration; some fevers completing theirs in twenty-four hours; others in forty-eight, while others require seventy-two, and some even ninety-six hours. The form which a fever assumes, in this respect, is called its *type*; so that a fever which occupies twenty-four hours, from the commencement of one paroxysm to another, is said to be

of the quotidian type; while one which revolves every forty-eight hours, is of the *tertian* type; and when this period is extended to seventy-two hours, the fever is of the *quartan* type; and a period of ninety-six hours constitutes the *quintan* type. The *quotidian*, the *tertian*, and the *quartan* types, constitute the three principal and primary types of fever; all of which are, however, subject to modifications which may readily mislead the careless observer, so as to confound them, or mistake one for the other, especially the quotidian and the tertian.

“It has been observed, that in fevers of the quotidian type, the paroxysms generally come on in the *morning*—a circumstance, which has been almost invariably verified in my own experience, and which is, indeed, so constant, that Cullen was induced to notice it in his definition of a *quotidian*. *Tertiens* commonly come on toward noon; but they are much less regular, in this respect than fevers of the preceding type. *Two* simple tertians sometimes go on cotemporaneously in the same patient; so that, instead of the paroxysms recurring only every other day, they occur daily, as in a quotidian. These cases are called *double tertians*, and are distinguished from quotidians by the paroxysms of the alternate days being similar in relation to the precise time of their occurrence, grade of violence, duration, and other circumstances. Thus the paroxysms on the *odd* days, will perhaps recur at nine o'clock in the forenoon, while those which happen on the *even* days will come on at two or three o'clock in the afternoon, so that, although each day has its paroxysms, the fever cannot be properly considered as a quotidian, but the cotemporaneous progress of two simple tertians, the one having commenced a day sooner than the other. Fevers, however, rarely assume the double tertian type from their commencement. They usually begin and continue for some time in the simple tertian type—the duplication occurring afterward; and when the type thus becomes doubled, the new or accessory paroxysms are in general considerably milder than those of the original or simple tertian. It is asserted, that a double tertian seldom terminates without first assuming the single tertian type, the accessory or weaker paroxysm disappearing first.”

CONTINUED FEVER.

Introduction.—It will be remembered that we divided the various forms of fever into two orders, and the first of these again into two genera—the *continued* and the *periodic*, and although this arrangement has many practical advantages, yet it is not strictly true. It appears, so far as observation has extended, that the functions, of every variety of organization,

are subjected to a species of periodicity, and this law is common to both health and disease. Consequently it is exceedingly rare to witness any variety of febrile action which does not, to some extent, indicate the presence of this law. And yet the decrease or abatement of febrile action, in this genus, is so inconsiderable, when compared with the other, as to leave no room for a mistake in the diagnosis.

The modifications of this genus are so numerous and prominent as to have led to considerable confusion among writers, and the names which the several modifications have received, are almost as numerous as have been the writers. The two extremes of this genus, as relates to the energy of their action, has caused it to be divided into two species the *sthenic* and *asthenic*, or *inflammatory* and *typhous*. That there is a very great difference between the highest grade of the sthenic and that of the asthenic no one will question, but to regard a fever as inflammatory that originates in general obstruction, is equivalent to contending for an impossibility; nevertheless, as a name, it is now understood, and hence we shall not depart from its accepted use. In order to convey some idea of the grades of febrile manifestations, they have been divided into three varieties: *synocha*, *synochus*, and *typhous*. The first is intended to embrace those febrile forms which have been denominated inflammatory; the third is intended to embrace those which exhibit the lowest degree of febrile action, and the second those of an intermediate character. The first has been again divided into two sub-varieties: the *idiopathic* and *symptomatic*. This division is another that inculcates a false idea, because both are, in truth, symptomatic. The first is symptomatic of a reduction of nervous irritability and a constriction of the capillary vessels, and the second, of a pre-existing inflammation or local lesion of the solids. To distinguish the former from the latter after this explanation, we may use the word *idiopathic*.

TREATMENT.

The treatment of this form of disease is very simple, as a general rule, if there be no complication; but it is not always practicable for the practitioner to determine immediately the existence of a complication, hence the safer course will always be to adopt an early treatment.

The first indication is to remove morbid accumulations in the stomach, and equalize the circulation and nervous action. To fulfill this, an emetic must be administered as early as possible, to be followed by a cathartic; after which, vapor baths, sudorifics, and refrigerant diuretics, must be given, and the surface bathed frequently with cold or tepid water. The wet sheet may here be used with advantage.

As soon as the more active inflammatory symptoms have become subdued, tonic febrifuges must be given; and here we prefer our usual preparation of Sulphate of Quinine, and Prussiate of Iron, which will generally overcome this form of disease, except it be complicated, when a course of treatment must be pursued which is adapted to the complication.

It will occasionally occur, however, that the inflammatory condition of the system will continue very severe and obstinate, not readily yielding to the above treatment; in such cases, the Tincture of Gelsemium must be employed every hour until the eyelids begin to feel heavy, with scarcely an ability on the part of the patient to control them, when its administration must be omitted, and no more should be given until the effect has entirely subsided.

To remove the accumulations, and produce an alterative action, let the Podophyllin, Leptandrin and Jalapin be given in sufficient doses to produce the desired result. If an active cathartic is necessary at the beginning, increase the quantity; when a moderate or alterative only is necessary, diminish the dose. This course should be maintained until the heavy coat is removed from the tongue, as well as the bad taste from the mouth.

If the tongue indicates much gastro-intestinal irritation, cathartics must be used with great care; in no case persist in their use if vomiting is produced by the medicine, for it may be laid down as a general rule in practice, that injury results from persisting in the use of any irritating agents.

The emetic when used should be efficient, and as a general thing should be resorted to in the very beginning.

After the emetic and cathartic have been used, and the stomach freed from nausea, let the patient take the Tincture of Veratrum Viride in as large quantities as possible without producing nausea or vomiting; this, however, is to be given when the pulse continues full and hard; but if there be any inflammation, or even much irritation of the stomach, vomiting will ensue at once; then the quantity must be reduced, or its use discontinued entirely.

CHAPTER V.

PYREXIA STHENICA—SIMPLE INFLAMMATORY FEVER—THE SYNOCHAL GRADE OF IDOPATHIC FEVER; FEBRIS VASORUM.

THIS form of fever most generally seizes young persons in the flower of their age, and full of blood, those who are athletic and therefore fibrous and elastic, finally, those who constitute the first class of organizations. It may occur at any season of the year, but it is more liable to prevail during the spring and summer.

It begins with a sense of lassitude, the body feeling as if bruised; with weakness; cold and hot sensations which alternately succeed each other; with tremors and with pains all over the body, but particularly in the shoulders, back, knees, legs, and head. The bowels are constipated and the secretions are suspended. As this stage passes off, the second is introduced with an intense and burning heat, an unquenchable thirst; a suffusion of the eyes, a redness and swelling of the face; nausea; vomiting; restlessness; anxiety; a full, quick, and strong pulse; a dryness of the skin; a scanty and highly colored urine; a rough and dry tongue, frequently covered with a thick scurf; the respiration is difficult and labored. Delirium is not very common to this form of fever, but when it does supervene it is severe and threatening; the acuteness of the senses of seeing and hearing is much increased, and finally, the blood, when drawn, shows a buffy coat, cupped crassamentum and a deficiency of serum; but the introduction of this symptom is measurably useless to Eclectics as they never see it, except, possibly, by some unexpected accident.

It is not uncommon for the preceding symptoms to manifest remissions and exacerbations—the first appearing in the morning, and the second in the evening, until terminated by some critical evacuation and a return to health; or by stupor, coma, tremors, twitching of the muscles, hiccough, and death.

This form of fever, when its simple form is maintained, very seldom continues beyond the ninth day, but it may be extended to the fourteenth, and it may conclude on the fifth or seventh. When the symptoms have continued to increase in violence to the fourth or fifth, a crisis may be looked for on the seventh, and when its crisis is procrastinated to the fourteenth, it will be observed to increase in violence to the ninth or eleventh.

Prof. Eberle, says that revolution in this form of fever is almost invariably *attended* by general and free perspiration, urination, etc. As this is one of nature's contrivances, he does not seem to think that the revolution is *affected* by secretion, of which perspiration and urination are now two of the most important. And here we would have the reader remark that he and most other authors inform us that a reddish or pale sediment of the urine, is a never failing concomitant in the crisis of this fever. Is the subtraction of this sediment from the blood a mere circumstance, and one of no particular importance? Did its retention in the blood have no agency in the production of the fever?

It should be remembered that in this form of fever the chilly sensations during the forming stage, do not appear to be attended by any absolute reduction of temperature, at all events, if such be the fact, it is not ascertainable by the sense of others, or the thermometer. The only opinion we can suggest to account for it is, that the cold and hot sensations which are alternately produced, are occasioned by alternate contractions and relaxations of the capillary vessels; that in the former no caloric is evolved, and that in the latter it is. In support of this view, it should be remembered that the system is now struggling to remove all constriction and finally succeeds.

In many constitutions of even the firmer class, there is some part more feeble than the balance of the system, and hence, therefore, more liable to take on inflammatory action in the progress of this fever, because of its inability to resist an excessive invasion of the febrile action. Hence it is that the simple and even tenor of this fever is frequently complicated with topical inflammation.

CAUSES.—Of all the causes to this form of fever, an undue or unguarded exposure to cold is confessedly much the most frequent, and in a large majority of instances this exposure results from an incapacity to anticipate atmospherical vicissitudes or mutations, and hence its more frequent prevalence in the spring or fall months, more particularly in cold or variable climates. When we consider the fibrous and sensitive character of those who are most organically liable to this form of fever, we should reckon an electric condition of the atmosphere as a frequent cause, hence the reason why northwest and

northeast winds so frequently occasion it. A sudden exposure to cold water is said to cause it when the person is heated by exercise.

An exposure to solar heat, by those of this class who have not been accustomed to it, frequently proves a successful cause. The use of highly stimulating food and drink, is also a frequent cause in old and affluent communities. But in all our reflections upon the etiology of fever, it should be remembered that the various grades and modifications of it depend more, much more upon the inherent organic conditions of the system, than upon either the remote or exciting cause. Hence we may safely assert that all the forms of fever which are incidental to any given section of country, at the same season of the year, are but so many varieties or modifications occasioned by the existing differences that organically exist between individuals. Thus, while the athletic—those having large organs of animal sensibility and muscular motion, will have pure synocha; another with less endowment of animal sensibility, synchus; and in a third, in whom both of those vital forces are feeble, a typhoid, which, by neglect or improper treatment may degenerate into a proper typhous. A sight of the patient, therefore, should generally determine the grade of the fever.

DIAGNOSIS.—It is distinguished from typhous fever by its strong, high, full pulse, dry tongue, intense thirst and heat, and greater violence of the pains. The absence of clearly defined periodicity, will distinguish it from those forms which constitute the next genus, and from the succeeding species by the diagnosis which will there be given.

PROGNOSIS.—Such is the vital resistance in synochal fevers that the prognosis should not be considered as either uncertain or unfavorable. What the bleeding, purging, and mercurial practice may effect upon it, is altogether another question. This, so long as it has no complications to contend with, is the least dangerous of all the forms of continued fever, and for the simple reason that the vital powers are adequate to the removal of the disease.

If the system, though generally strong, should possess feeble parts, inflammation, in some one of them, may supervene, and in such case, the magnitude of the danger will depend upon the functional importance of the invaded part. If the breathing should become obstructed or painful, or attended with a cough, or with pain in the thorax; or if the abdomen be tense and tender upon pressure, we may apprehend danger. The presence of mere delirium should not be regarded as unfavorable, but should the existence of violent pain in the brain become indicated, encephalitis may be suspected to exist and to greatly increase the danger.

FAVORABLE SIGNS.—A slight hemorrhage from the nose, a general perspiration, urine pale when voided and turgid when cool; a reduction of the temperature and of the hardness, quickness, and frequency of the pulse.

SYNOCHUS FEVER—COMMON CONTINUED SYNOCHUS FEVER—ENTERIC FEVER—TYPHOID MITEOR—TYPHOID FEVER—ENTERO—MESENTERIC FEVER, ETC.

WE have thought it best to regard this form as a distinct species, for the reason that it is confined to those regions of the country in which the bilious or so-called miasmatic forms of fever do not prevail.

It is very probable the several names, at the head of this article, have been given to one and the same form of disease, modified probably by local and constitutional circumstances. The profession, it seems, have been brought to this conclusion by the labors of Louis, who designated it by the name of "typhoid fever," but its nomenclature is still unsettled, and consequently we have preferred to call it by a name which indicates a grade of action between the highest and the lowest that is known to the genus. Dr. Wood calls it "enteritic fever," and probably for the reason which we infer from the following remark of his: "The intestinal affection is as characteristic of this disease, as the eruption is of small-pox." We can not adopt this name, because it only designates a *specific* form, instead of a general one. We conceive that fifty individuals may have the same grade of continued fever, and yet no two of them will be attended by the same local or special phenomena. The multiplication of names to include or express local or incidental circumstances, has done much violence to the profession.

We are disposed to suggest, in this place, that the present form represents the general character of this genus, and that the synochal form is an upward departure from it, and that the typhous is a descending one; and hence they should be regarded as being merely varieties, and this conclusion is sustained by a corresponding condition of the vital forces among men. Some men are much above the average of society in vital force, and others are much below it; the average then, is the legitimate sphere of the synochus grade of fever. Either of the ascending forms of constitution may have typhous fever, because their vital force may be reduced to the typhous level by previously debilitating causes; hence it is that every man in a crowded and filthy ship or prison may, finally, die of typhous fever. But the reverse of this can not be effected—a man of a low grade of vital force will never have synochal fever.

Now, in that mass of society which determines the average of the vital force, there necessarily exists many modifications, and each epidemic may result from a modified condition of the atmosphere. In one season, it may have a particular pulmonary, nervous, or gastro-enteritic tendency. Under these views, we think it best to generalize febrile forms of disease, to the full extent of practical advantage.

This, or the synochus grade of fever, may have the usual forming stage or period of fever, but it frequently gives very little or no premonition of its approach, and abruptly announces itself by a chill, which may almost as abruptly terminate by the introduction of the fever; but most frequently it is imperceptibly merged or lost in the fever, which so gradually increases as to leave the patient unconscious of the moment of beginning.

In some instances, again, the patient is scarcely conscious of the existing chill, and the fever that succeeds is of a very mild grade; the tongue becomes white, the pulse more frequent and full, with a dry skin; pain, more or less severe, over the eyes; urine diminished and highly colored; sleep disturbed, and bowels constipated. The appetite is not always arrested. When the affection is of this mild grade, it passes off in a few days by perspiration or diarrhea.

In our cold and variable climates, the ordinary continued forms of fever present various degrees of violence, from the mild form above described, to those of cerebral oppression and fatal collapse. Gastro-intestinal irritation may almost be regarded as a pathognomonic symptom of this form of fever, and it usually appears early in the seizure, by the manifestation of nausea, vomiting, unpleasant gastric sensations, and foul tongue.

In that form which is most usually denominated continued fever, the symptoms are, from the beginning, of a pretty high grade of violence. Beside a distressing degree of the symptoms of the forming stage, the exciting degree of the hot is pregnant with threatened danger. The skin is hot and covered with a pretty uniform, but slight, blush of red; the face is flushed, the pulse is active, full, and frequent; the patient is restless, and probably peevish, and apparently incapable of fixing his attention upon anything, except a dull, heavy, or throbbing pain in the head. The urine is sometimes pale, but most frequently it is red, and entirely destitute of sediment. The tongue, being white in the beginning of the fever, becomes, in its progress, dry, harsh, and dark-brown; the bowels, as usual in fever, become torpid, but the feces continue soft, but frequently contra-indicate the presence of bile.

For six or seven days, these symptoms, with slight intermissions and exacerbations, continue about the same; but, in

the further progress of the disease, cerebral symptoms are apt to supervene, more particularly delirium at night. If the vital energies are capable of effecting a favorable termination, it may be expected on the eighth or ninth day, otherwise it will pass, with more or less rapidity, into the typhous form or into collapse, when all the symptoms will become of a very threatening character—such as delirium, more or less stupor, dilated pupils, foul and dark-brown tongue, hurried breathing, picking at the bed clothes, subsultus tendinum, and a gradually sinking pulse to the close of life, which may happen on the fifteenth day, but it may be procrastinated two or three days longer.

When the cold stage has been protracted, and reaction established with difficulty, we may infer the existence of a feeble vital force, and therefore we should be prepared to expect a rapid declension of the disease into the typhous stage, attended with exhibitions of cerebral disorder of no ordinary grade—such as an aversion to light and sound, a disposition to watchfulness, confusion of ideas and delirium, which may become continuous and violent. The head is rolled from side to side; the arms and feet are kept in frequent motion; all portions of the body are tender to the touch, and evanescent pains are felt in various parts of it. There is apt to exist, also, tenderness at the epigastrium, flatulence and irritability of the stomach. In this modification of the disease, the pulse never becomes remarkable for tenseness or fullness, and the typhous condition of the disease may be expected to supervene on the fourth or fifth day, and then we have a reduction of some symptoms, and an introduction of others of a less favorable import—the delirium passes into a low and muttering raving—great mental oppression, and a rapid destruction of the vital powers.

When synochus fever occurs under circumstances of debility arising from poverty and depravity of living, such as may characterize life in a damp cellar, poorly ventilated and warmed, with food poorly calculated to produce health, then we have a fever in which the prodromal stage is much protracted, and the hot one extended indefinitely to five or six days, or probably to as many weeks. The pulse, in activity, frequently continues at very nearly the normal standard, but usually a little accelerated. The appetite is absent or greatly impaired, the thirst is very moderate, the eyes are dull and suffused, and the patient drowsy; the tongue is white and slimy, the urine is small in quantity and pale, containing, most generally, much mucus. This form does not necessarily run into the typhous state; but, as it progresses, the pulse becomes smaller, weaker, and more frequent. It may terminate favorably about the seventh or ninth day by a general diaphoresis, with a slow

convalescence. In the case of a fatal termination, the event will be preceded by muttering, subsultus tendinum, hiccough, and coma.

When those persons who have been exposed so long to those influences which are called miasmatic, as to have the nervous system more or less enfeebled, and the secretions more or less impaired, become exposed to the influence of cold, a common cause of synochus, a modification of it will be induced which demands special notice. Beside the usual symptoms of the prodromal stage of fever in general, we are required to add a sense of distention and weight in the stomach, acid or bitter eructations, an icterode complexion of the face, pains in the abdomen, constipation or a bilious diarrhea, occasionally chilly sensations, and a silent and gloomy disposition of mind. In the hot stage, the pulse is but rarely over 112, during the first several days, and for the same time it is full, active, and compressible; the skin rarely becomes very hot—the tunica albuginea and the whole surface of the body become of a jaundiced hue; some moisture may be frequently observed, during the course of the fever, about the region of the heart, but a general perspiration is but rarely, if ever, witnessed before the final resolution of the fever. The urine is small in quantity and charged with bile; the tongue is bitter and covered with yellowish slime, which becomes brown as the disease advances, and, though moist in the beginning, it becomes dry, hard, and of a brown color. Nausea, vomiting, and a loathing of food always attend, with a desire of drinks, particularly of an acidulated character. In this modification there is an early tendency to pass into the typhous form, consequently the most prompt attention should never be withheld. The breathing is usually oppressed and attended with some cough; the remissions and exacerbations are quite apparent. If it shall reach the typhous stage, then delirium and all the other phenomena that usually characterize this form of fever will be apt to make their appearance by the fifth, seventh, or ninth day.

In the course of this malady there are certain symptoms, that occasionally arise, which so unavoidably affect its general character as to make it expedient that we should notice them.

Diarrhea is a frequent attendant upon continued fever, but, whether present or not, such is the gastro-enteritic irritability that the bowels are much more easily operated upon by cathartic medicines than in other forms of fever; and where a doubt exists as to the character of the fever, this circumstance in the opinion of Dr. Wood, should have weight in determining our opinion. He says, that diarrhea sometimes precedes the fever, but more frequently comes on during the first twenty-four hours, and is often procrastinated to a later period. The

evacuations may be only one or two per diem, and they may be extended to a dozen or more; and what is equally if not more remarkable, they have a normal or healthy appearance, except as to consistency. As this peculiarity attends throughout the disease, it becomes one among the best of the diagnostic symptoms, and, furthermore, the liquidity of this excretion being common to this form of fever, marks it as one of the most obstinate, and therefore, dangerous forms of fever we have to contend with. It prevents that concentration of the vital force which is indispeusable to the re-establishment of depuration. The disease is sometimes attended, particularly in its advanced stages, by dark-colored or even black stools—sometimes by such as indicate the presence of inflammation, and abdominal pains are not unfrequent attendants.

Tympanites.—Dr. Wood states that he has scarcely witnessed a case of this fever in which this symptom did not more or less prevail. It does not become very obvious until about the seventh day, but sometimes it obtains as early as the third. Its violence is usually proportioned to that of the disease. Sometimes the abdominal distention occasioned by it is very distressing—it greatly interferes with the respiratory function. It appears to be mostly confined to the large intestines.

Rose-colored eruption is said to be very characteristic of this form of fever. It occurs most frequently between the seventh and fifteenth day, and first appears on the abdomen, from which it extends to the breast and inferior extremities. It appears in small, red spots, either round or oval, and about a line in diameter, more or less, and possess a slight elevation which disappears upon pressure, and returns upon its removal. Their number is sometimes only two or three, but in many instances they are innumerable. By appearing in successive crops, they occupy a period ranging from three to fifteen days. They are said to be very rarely absent. They are easily distinguished from petecchiæ, which consists of extravasated blood in the skin.

Sudamina.—This consists of vesicles not larger than a pin's head, and in order of time their appearance is subsequent to that of the rose-colored eruption. They are more readily felt than seen, unless viewed obliquely. They may spread over the entire surface, except the face, but they are more usually confined to the neck and the superior portions of the chest. They have but little diagnostic weight, as they occur in other forms of fever, and by no means constant in any.

Cough and bronchial rales.—Dr. Wood says, that "this is a very common complaint," and that the "cough is either dry or attended with a slight mucous expectoration, with very little or no soreness or sense of oppression in the chest. The dry, sonorous, and sibilant rales may be heard more or less exten-

sively over the thorax, and are much greater in proportion to the amount of oppression or dyspnoea than in ordinary catarrhal affections. They thus afford an important diagnostic sign. They are not, however, present in all cases. Sometimes they begin with the disease, but more frequently not until the lapse of about a week. Occasionally they give place to a crepitant or sub-crepitant rale, indicating the occurrence of inflammation in the parenchyma of the lungs."

Pulse.—This source of information to the vascular condition of all forms of disease betrays much variety in this form of fever. It presents great diversity of character in different cases; sometimes, in the early stages, it is full and strong, and sometimes feeble from the outset, and always becomes so in the progress of the malady. But that average pulse, which is most characteristic of this fever, is one of frequency, smallness, and feebleness, and is indicative of nervous irritability and capillary congestion. Its general range of beat is from 110 to 160; the former marks the beginning and the latter its last stage. When the system passes into a prostrate condition, its frequency is reduced below the standard of health.

Hemorrhage.—In the early stage of the disease epistaxis is not an unfrequent phenomenon—the result, perhaps, of an active determination to the head; it is generally slight, though sometimes requires attention. In the advanced periods of the disease, hemorrhage from the bowels is apt to supervene, and as it is passive it may be always accounted a dangerous indication.

Dullness or Hebetude.—In the early and probably most congested period of the disease, the countenance of the patient becomes as it were a blank—he is apathetic and low-spirited. This dullness may increase with the disease, until stupor and even coma may supervene.

Delirium.—This usual attendant upon continued fever may appear early in the disease, but it is usually procrastinated until about the seventh day. The manifestations of this condition of the brain, embrace every possible conception of which the mind is capable. They constitute a field of much study and interest to the phrenological physician. The peculiarities of the delirium are always in harmony with the prevailing endowments of the brain. The strongest feelings or most governing trains of thought are pretty certain to be exhibited. When the vital and lower animal faculties are strong, the delirium may be furious; if caution be strong and hope small, it will be of a desponding character; if the social sentiments be well endowed, the patient will betray great pleasure in his vivid conceptions of the beautiful and the agreeable; and if he has much language in connection therewith, he will probably write poetry on the wall, or recite it to his attendants. We

once attended an unlettered but a very philosophical man—who was organically a philosopher. During his fever, he appeared to find exceeding pleasure in the solution of philosophical questions.

But this state of mental exuberance is not of long continuance—it passes very frequently into a muttering of broken sentences, and thence into stupor, and finally coma. It is proper to add furthermore, that there are two varieties of delirium in this fever—one results from an active cerebral circulation, in fibrous constitutions, and the other from cerebral irritation, resulting from an insufficient circulation—debility. It may be further remarked, that, with the remissions and exacerbations, these two forms of delirium may alternate each other.

Sloughing of the skin.—The physician should be constantly on the watch to prevent this liability from running into serious mischief. Vesicated portions of the skin and those upon which the patient constantly reposes his weight, are very apt to slough. The knowledge of this tendency should greatly direct our indications of treatment—diminished vitality, and consequently a feeble capillary circulation.

Retention of urine.—The accumulation of urine in the bladder sometimes becomes very considerable—the distention occasioned in this way has sometimes been enormous. The possibility of this occurrence should keep the physician admonished of his duty in this respect. “M. Marten Solon,” says Dr. Wood, “has paid special attention to the state of the urine in this disease. He has found it to be more scanty, higher colored, and denser than in health, equally acid, if not more so, much more abundant in urea, and occasionally aluminous, especially in severe cases, in which this character of secretion must be considered as an unfavorable sign.”

We have now passed over those symptoms which more or less frequently obtrude themselves upon this form of disease, if not absolutely essential to it. As they are well calculated to keep up the vigilance of the physician, we could not consent to omit them.

CAUSES.—Upon this subject there prevails much more speculation than knowledge. It is known that cities, prisons, hospitals, camps, and ships are, perhaps, more frequently visited by it than other situations; but it is by no means peculiar to them, for it has fatally prevailed, as an epidemic, in the healthiest regions of our country. It is known, furthermore, that those who are under the meridian of life are much more liable to it than those who are above it. Dr. Wood says, that “of 255 cases observed by Louis and by Chomel, 78 were from fifteen to twenty; 95, from twenty to twenty-five; 54, from twenty-

five to thirty ; 22, from thirty to forty ; 5, from forty to fifty, and only one above fifty."

These statistics have induced the general opinion that the aged are less liable to it than the youthful ; but as to the reason why this is the fact, no one seems ever to have ventured an opinion. Having pretty fully explained such facts in the second book, we will just remark here for the purpose of calling attention to it, that the least viable are the most liable to be cut down first. No one ever died at twenty-five, who had the vitality of those who are now living at the age of eighty-five years. We are willing to admit, however, that the two extremes of life are less liable to it, than that period which is the most distinguished for animal sensibility and vascular action.

It has been thought to be contagious, but the weight of professional opinion is now opposed to such a conclusion. Dr. Wood has advanced an opinion which gives direct support to those expressed by us, in defining the constitutional difference between this and other forms of continued fever ; but the reader would scarcely notice the correspondence, without a special call of his attention to it, because there is no similitude in the modes, respectively, of reaching the same fact. He says :

"On the whole, the most rational view of the etiology of enteric fever, in the present state of our knowledge, seems to be, that an inherent predisposition to this disease exists in many persons, analogous, in some measure, to the tuberculous, the gouty, and the rheumatic predisposition, which is liable to be called into action by various exciting causes, perhaps by almost any cause capable of considerably disturbing the vital functions, but that all persons do not have the predisposition, and that it is generally exhausted by one attack of the disease."

If, now, the word predisposition, in the preceding quotation, be replaced by the word *liability*, then the agreement between us becomes obvious ; with this difference : we have exposed the organic conditions upon which the liability depends, while he has not and can not show upon what a predisposition depends. We have shown that a predisposition is, absolutely, disease—a conclusion which he would not grant, and yet it is one which he can not avoid.

Nature.—Authors have given us nothing on this subject which we are willing to honor with the epithet of *respectable nonsense*. Many have labored to find some sort of inflammation in some sort of a place in which to found it ; but, as yet, they have so utterly failed, as to cause Dr. Wood to conclude : "As to the real nature of the fever, we are in the dark, as we are, in fact, in relation to all the essential fevers."

We are of opinion that fever is as easily understood as inflammation, and that it is as comprehensible as any other vital action; and as to the ultimate nature of any one, we neither know nor can know anything. Fever, in every instance, must hold a precise relation to the character of the disturbing obstruction and the organization its acts upon, and the mode of its action.

Diagnosis.—It should always be remembered by the attending physician, that all febrile forms have, in the beginning, so strong a family likeness that it is exceedingly difficult, if not impossible, to distinguish one from another; consequently, no attempt should be made at diagnosis for the first three or four days. If an epidemic prevail, he will be justified, in the beginning, in treating it as one of the same form. After the lapse of three or four days, the soft, or even the semi-fluid character of the feces, the insidious character of the attack, the gloomy expression of the countenance, the cough, the rose-colored eruption, the tympanitic abdomen, the sudamina, the retention of urine, and the pulse will distinguish it from other forms of continued fever. From remittent fever, it may be distinguished by the less marked character of its remissions. When remittent fever assumes the typhoid character, the diagnosis will become obscure, and of but little importance if made. The diagnosis between it and typhous will be presented when we treat of the latter.

Prognosis.—When this form of fever is allowed to run its course, without becoming complicated with local inflammation, it should never be accounted dangerous, but as one, particularly under a proper treatment, disposed to terminate favorably by a diaphoresis at some time within the period of two weeks. But sometimes the constitutional infirmities do not permit of such a result, and then we find the pulse weak, small, and frequent, the muscular system greatly prostrated, and a constant disposition of the patient to lie upon his back, all of which denote a collapse, from which recovery is scarcely ever to be expected, and more particularly if complicated with inflammation, which is most generally the case. Under such circumstances, the prognosis is always unfavorable.

Indications.—In this form of fever, nothing more appears to be required than to remove the constriction, to equalize the circulation, and to sustain these achievements when gained by a proper alterative course.

Treatment.—At the commencement of this disease, we prefer the use of anti-periodics, anodyne, and diaphoretics, for the purpose of allaying nervous irritability, equalizing the circulation, and thereby removing constriction. If, however, diarrhea should be present, or an extreme susceptibility to such condition, it will be proper to commence the treatment with

mild evacuants, such as the Compound Powder of Rhubarb and Potassa, or the syrup of the same, in doses of five or ten grains of the powder, or a fluid drachm of the syrup, to be repeated every two hours, and continued until the bowels are thoroughly evacuated, and the diarrheal disposition removed or lessened. Occasionally, when hepatic fever is present to a considerable extent, we prefer the following preparation:

R. Podophyllin, grs. ij,
Caulophyllin,
Leptandrin, *aa* grs. vi,
Comp. Powder of Ipecac. and Opium, grs. xvi. Mix,

Divide into four powders, and administer one every four hours, until catharsis of a bilious character is produced. This compound may likewise be used in cases where the diarrhea is accompanied with watery evacuations and much pain.

After having accomplished the results to be desired from the above agents, it becomes necessary to administer the anti-periodic and diaphoretic agents at first referred to; the most efficacious combination we have found to be as follows:

R. Sulphate of Quinine, grs. xx,
Ferro-Cyanuret of Iron, grs. xv,
Cornin, grs. xviii,
Hydrastin, grs. xx. Mix,

Divide into ten powders, and give one every four hours, in any convenient vehicle, and it may be continued throughout the disease, if not subsequently contra-indicated, even during the stage of convalescence.

In connection with this powder, and for the purpose of promoting diaphoresis, the Compound Powder of Ipecac. and Opium must be given in doses of from three to five grains, three or four times a day; this, however, must be omitted should there be pain in the head or a determination thereto.

In every case of this fever, throughout its whole course, especial attention must be bestowed upon the surface. It should be bathed as often as two or three times a day with a cold alkaline wash, and even more frequently, should the heat obstinately continue. The patient may drink freely of cold water throughout the whole course of the disease, and if he desires it, a piece of ice may be given him. By a perseverance in this treatment, the disease will generally yield in from three to eight days.

In the progress of this fever, many symptoms of a local character may present themselves, demanding the prompt interference of the practitioner. The diarrhea may not give way to the above treatment, but may continue with much severity,

rapidly debilitating the patient; in this case, astringents must be given, and we prefer the addition of from five to eight grains of Geranin to each dose of the above Rhubarb and Potassa preparations, to any other agent with which we are acquainted.

Tympanitis is a common occurrence and must always be treated with warm fomentations, emollient cataplasms, and counter-irritants, together with the means heretofore named for this local difficulty. If it should obstinately continue, with constipation, the injections referred to in another part of this work, must be often repeated with laxative doses of Seidlitz Powders.

Cough is sometimes present, and may be relieved by the use of equal parts of the Syrup of Squills, Syrup of Senega, and Camphorated Tincture of Opium, and of which one or two fluid drachms may be administered every three or four hours. If it proves severe and unyielding, Mustard Poultices must be applied over the chest.

Hemorrhage frequently occurs in this disease; when moderate it is not alarming, but becomes so when excessive. It must be met by the continued use of tonics and astringents; and if it be excessive epistaxis, it may be become necessary to plug the nostrils with sponge, moistened and dipped in powdered Tannin.

Under the old depletive system of medicine, the condition of the blood and of the nervous power becomes so changed by the treatment, that this symptom is not an unfrequent attendant; while, under the Eclectic practice, in which the integrity of the system is maintained throughout the whole course of the disease, the corpuscles of the blood being increased, and the nervous system invigorated, it is seldom met with.

Should there be excessive determination to the head, as manifested by pain, or delirium, or both, cold applications must be applied to the head, with counter-irritation to the back of the neck, as the application of Mustard Poultices; and the inferior extremities must be kept very warm by bathing in hot water, application of bottles of hot water, or other similar means.

When retention of urine occurs, warm fomentations of bitter herbs applied over the pubes, with diuretics internally, will generally prove sufficient. The diuretic preparation which we most commonly employ is Spirits of Niter, given in half-drachm doses, every hour or two, in Spearmint tea, or decoctions of Marsh Mallows, or Uva Ursi. This treatment is recommended where sensibility exists, as evinced by pain; but as this symptom frequently occurs without pain, the practitioner must be watchful of it, and in such instances introduce the catheter.

In the advanced stages of the disease, when there is debility, with stupor and delirium, stimulants must be given, and continued, until the solution of the disease, and until convalescence has ensued; wine whey, Longworth's Catawba wine, porter, and even brandy may be administered at intervals with excellent effect. Sulphuric Ether is often efficacious in cases of sudden and great prostration. The temperature of the system must also be regulated as much as possible, by the application of sinapisms, hot bricks, or bottles of hot water, etc., kept constantly to the feet and legs, and cold or cooling washes applied to the head. Excessive stimulation to the lower extremities must be avoided, lest sloughing should take place. If the coma or delirium be obstinate, the hair may be cut or shaved off, and the cooling washes continued.

The diet of the patient, in the early part of the disease, should be very light and usually of a liquid character, as toast-water, solution of Gum Arabic, rice-water, weak Indian-meal gruel, together with the juice of sweet grapes and oranges.

As drinks, cold water, cold lemonade, or infusions of bitter herbs may be allowed. In the stage of prostration, animal broths, beef or mutton tea, milk punch, and other stimulating nutriment must be given to sustain the patient's strength.

The room should be frequently ventilated, and the clothing changed at least as often as every other day.

During convalescence, the diet should be mild, nutritious, and easy of digestion—care being taken that the patient does not indulge too freely, or use indigestible food, lest sudden dissolution should happen.

TYPHOUS FEVER—TYPHUS GRAVIOR—SPOTTED FEVER—PETECHIAL FEVER—PUTRID FEVER—CAMP FEVER—SHIP FEVER—JAIL FEVER—HOSPITAL FEVER.

In our treatment of febrile forms of disease, we shall observe the distinctions which have been made by Dr. Wood. The words *typhous* and *typhoid* will be used as adjectives to express that resemblance of typhous fever, which other forms of fever sometimes assume; while *typhus* will be used as a substantive or as a name to designate a special form of fever—that which has been denominated typhus gravior, spotted fever, ship fever, etc.

When treating of synochus or typhoid fever, we gave it as our opinion that synocha, synochus, and typhus were but three varieties, but the differences between them are such as to have caused them to be, by almost all writers, treated of as three pretty distinctly-marked forms of febrile disease. We have, therefore, arranged them as species, more especially as there exists many intermediate shades of difference between

the three, which constitute varieties. This arrangement, as we have before remarked, has its parallel in the three grades of vital strength that distinguishes society. To the word *predisposition*, we can attach no idea independently of an obvious organic condition—as an abstraction, it conveys no idea, except that of the fact—the patient has a fever or is liable to it; and of this liability it conveys no instruction. But the liability of one to a particular form of disease is indicated by an observable peculiarity of organization, and thus our idea is rendered intelligible and instructive.

Dr. Wood says: “There are undoubtedly strong resemblances between certain cases of the enteritic (synochus) and typhous fevers; and though in most cases easily distinguished by the obvious symptoms, they were often confounded, until the work of Louis placed us in possession of the means of a more accurate diagnosis.” He continues: “But it is highly probable that the two diseases are sometimes combined, in consequence of the simultaneous character of their causes.”

This conclusion we very seriously doubt. It is very probable that the two causes could make equally strong impressions upon the same system at the same time; and it is equally improbable that two forms of disease could possess the system at the same time; but we can readily admit that two causes capable of producing, by their joint action, a modification of obstruction, might produce a modified form of fever.

But, whatever may be the cause or causes of typhous fever, we exceedingly doubt their capacity to produce it in one of a vigorous vital force—a strongly-reactive force, unless it has the power to strike with death, as it were, the irritability of the nervous system. The history of the disease shows that persons in comparatively good health have been suddenly and stricken with the disease; but of the condition of their vital force, normally, we have had no information.

If, however, the cause has this power; then this form of fever is distinct, and also independent, in a great measure, of all vital conditions. It is probably well known that when a typhous epidemic prevails, all other forms of disease betray a tendency to fall into the same type, just as we have recently known, under an Asiatic cholera atmosphere, all other forms of disease assumed more or less that of cholera. With these speculations, which are always justifiable in the absence of knowledge, we proceed to the symptoms of typhous fever.

CHAPTER VI.

Symptoms.—An intense, pungent and remitting heat of the surface; a tense, low, quick, and unequal pulse; extreme weakness; vast anxiety and dejection of mind, presaging everything that is bad; nausea, and, in malignant cases, a vomiting of black bile; pain in the temples and over the brows, a deep-seated pain in the eyes, and a great injection of them; a sort of smoke-dried complexion; ringing in the ears; a laborious respiration, interrupted with sighs; pain in the stomach, limbs, and back; a difficulty in lying down; tremors; delirium; the tongue at first white, afterward black and dry; the lips and teeth are lined with a viscid sordes; an unquenchable thirst; a bitter taste in the mouth; the urine, at first, pale, but as the disease increases, extremely high-colored, and in some very putrid cases, it assumes a blackish color and deposits a stool-like sediment; fetid sweats, which are sometimes tinged with blood; stools, highly offensive, livid, black, or bloody; small, livid spots on the skin, like flea-bites, called petechiæ, or, if more extended, vibices, hemorrhages, aphthæ, ulceration of the fauces, and hiccoughs.

We have presented a dense group of fearful symptoms, and for the purpose of their elucidation a few words of comment and explanation will not be out of place.

The initial stage greatly varies in both degree and duration; it is in some instances so slight as scarcely to be observed, but in others it is severe and protracted; the skin is pale, cold, and shrunk; the expression is peculiarly anxious; the pulse is very feeble; and so great is the prostration, that reaction does not become established; but this extreme prostration is by no means a general feature of the disease. The hot stage, in the beginning, manifests no considerable departure from what is common to other forms of fever.

We have named nausea and vomiting among the symptoms, nevertheless they are not common — frequently entirely absent,

and the bowels are generally costive. This marks a diagnostic difference between this and the synochus fever, and we regard it as a more favorable symptom. The temperature of the skin ranges from 100 to 109 degrees, and the pulse from 100 to 160 beats in a minute; the respiration is also very hurried, and an exacerbation usually becomes manifest toward night, with a remission in the morning.

The disease having now been at its zenith for several days, its characteristic eruption then appears—that which originally gave to the disease the name of petecchial fever. This eruption is confined to no particular portion of the body—it may be quite local, and it may be so general as to impart to the surface a measled appearance. It may or may not be slightly elevated, and in color it may vary from red to black, as the disease progresses toward a malignant condition. The eruption appears usually from the fifth to the eighth day, but it may anticipate two or three days, or procrastinate four or five. Occasionally they disappear, and then reappear, and sometimes they are complicated with sudamina.

The tongue takes on a brownish color, is more or less dry, particularly in the middle; in many cases a dark sordes collects on the teeth, gums, and lips. It happens sometimes, as in the typhous stage of other fevers, that the tongue is clean and glossy, and finally comes to resemble raw beef. The appetite, in many cases, is totally lost, but the loathing of food is less than it usually is in more active fevers. The color of the face becomes of a dark-red or of a livid hue; the eyes become turbid; some hemorrhage is apt to take place from the nose, and a peculiar odor exhales from the skin.

As the disease advances, its characteristic stupor increases, so that the patient ceases to notice anything that is going on about him, and not unfrequently delirium is complicated with the stupor. So powerless is the circulatory system, that the slightest exertion is attended with syncope; great thoracic oppression not unfrequently attends this excessive debility. The malady has now obtained that stage in which a change must take place, and if it be toward recovery, the frequency of the pulse becomes diminished; the skin becomes relaxed; the tongue moist and cleaning; the eruption fades; stupor subsides, and consciousness returns. This change toward convalescence is sometimes distinguished, like synochus, by a copious perspiration or urination, and the patient falls asleep, to awake, comparatively, a new or different being.

But should the change be for the worse, then the patient lies upon his back, eyes half closed, mouth open, delirium with subsultus tendinum, hiccough, picking at the bed-clothes, difficult deglutition, involuntary discharge of urine, extremities cold, features collapsed, pulse, perhaps, absent at the

wrist. The physician should not abandon the case yet ; if he does, he may possibly learn on the next day, to his shame, that the system of the patient rallied through the night, and he is convalescent. When there is no necessarily fatal lesion of some very important part, it may be said, "where there is life, there is hope."

Convalescence from this form of disease, as might be expected, is slow, but it generally advances to good health with very little liability to relapses. The time occupied in running its course is very variable ; it may, in mild cases, conclude in recovery in a week, and in other forms of a more violent nature it may consume four weeks ; but, generally, one week is consumed in its advancement, one to its completed stage, and one to its decline. We should add, that in those cases in which reaction can not be established, death may supervene in twenty-four hours.

Causes.—About these we have some facts, but no philosophy. The most common resort of this form of disease is in camps, prisons, ships, hospitals, and, in private life, the filthy abodes of the destitute and miserable. It is inferred, therefore, that a poisonous effluvium is generated from the bodies and excrements of those who are thus confined together. Very many facts have been collected to render it very probable that the poison thus generated, may, through the medium of the atmosphere, visit the disease upon those who have not had any part in the generation of the poison. It is, furthermore, pretty generally believed that the poison can be confined in clothing, etc., and thus transported beyond the region of its generation, and re-produce the disease.

It is also believed to originate in epidemic influences. This, to us, is a puzzle, that the human body, under certain circumstances, and the atmosphere, shall produce a cause, which, in its results, shall be the same. Dr. Wood states, that he has no doubt that the fatal epidemics that prevailed in our country between 1807 and 1820 were of typhous fever. Both sexes and all ages seem to be liable to it, and the winter season, rather than any other, the time for its most frequent appearance and greatest prevalence.

Diagnosis.—This form of fever can, in a great measure, be distinguished from all others, by the sordes upon the teeth, gums, lips and tongue ; by the habitual stupor that attends it ; the suffusion or injection of the eyes ; the dusky or dark hue of the countenance, and great prostration of strength.

The typhoid fever is most frequently confounded with it, but from it, it may generally be distinguished by the difference in the condition of the bowels, respectively. In typhoid, they are very rarely habitually aperient ; while in typhous they are habitually constipated. Again, alvine

hemorrhage is common to the former, but very rare in the latter. The typhoid eruption differs from the typhous; tympanites is common to the former, but exceedingly rare in the latter, and the same is true of the bronchial rales, which generally attend the former. (See the article on Typhoid and Typhous Fever, by Dr. W. Jenner.)

Prognosis.—This should always be regarded as uncertain; but the appearance of diarrhea or fetid perspiration, in the declension of the disease, and a change of the petechiæ from a blackish aspect to a brighter color, may be regarded as favorable.

Dr. Eberle says, that spontaneous vomiting during the first and second day of the disease, more especially when the unpleasant cephalic sensations are thereby abated; slight hemorrhage from the nose, about the sixth or seventh day of the stage of excitement, is a good indication, and so is a moderate diarrhea at an early period of the disease.

The unfavorable signs are, a vast number of livid spots, a sudden disappearance of them, black aphthæ, no thirst, inflamed fauces, laborious respiration after the eruption of the petecchiæ, a swelling of the abdomen, with a concomitant diarrhea, highly fetid and ichorous stools, coldness of the extremities, and convulsions. To these Dr. Eberle has added a still more certainly fatal class: as blindness, involuntary flow of tears, difficult deglutition, paralysis of the tongue, insensibility to active vesicatories, and involuntary colliquative stools and hemorrhages. After all, he adds, patients do sometimes recover from this disease after many of the most alarming of these symptoms have made their appearance.

Indications.—The symptoms of this form of fever show that that chemico-vital action upon which the calorific function depends, is greatly arrested and deranged; that the vital laws show a constant tendency to yield to the chemical; whereby the entire blood mass is threatened with total disorganization; consequently the whole system should be toned up, heat should be applied, depuration should be promoted, and the putrescent tendency of the system should be counteracted.

Treatment.—In this form of fever, we pursue a similar course of treatment to that recommended in typhoid fever, with the exception that counter-irritants must be more energetically applied to the extremities and the spinal column, together with the use of antiseptics, when putrid symptoms are present, as Vinegar, diluted Pyroligneous Acid, Elixir Vitriol, etc. This condition will not admit of sedatives to any considerable extent, but very often permanent stimulents in combination with tonics have to be adopted from the beginning and continued throughout. Notwithstanding the frequency of the pulse, sedatives are not admissible, for this depends upon the nervous debility and not upon an inflammatory condition or high vital

action. If this condition should be mistaken and sedative or relaxing agents be used, the patient will soon sink under their employment.

GENUS II.—PERIODIC FEVER.

INTRODUCTION.—This genus embraces those febrile forms which are usually attributed to *miasmata*; and though some of them are not strictly periodical, yet their remissions are more marked than those we have treated of as being continued. The opinion is pretty generally entertained that those of this genus are but varieties—essentially the same—modified by the intensity of the cause and the constitutional peculiarities of the assailed; nevertheless, as it is common to treat of them separately, and as they admit of well-defined differences, we shall treat of them as so many species, and then ample room will be left for any number of varieties.

SPECIES I.—*Intermittent Fever—Fever and Ague*:

The symptoms which we detailed under the general name of fever, as applicable to the forming stage of fever, apply very accurately to the same stage in this. We will add one symptom, and so far as the writer has observed the phenomena of this stage in himself, it precedes all others, and frequently by a week or ten days; it consists of a sort of pungent or drawing pain, as though the epigastrium was being drawn to the spine, and for the purpose of relief he frequently took full inspirations. After he came to observe this symptom, in connection with this form of fever, he never failed to prevent its occurrence by the use of prophylactic measures. The pain is located immediately at the inferior extremity of the sternum. The writer has not found this symptom recorded by any one else, nor does he know whether it obtains generally or not; but if it does, its appearance will give ample time for prophylactic measures.

We have before remarked, that a paroxysm consists of three distinct periods in this form of fever: the cold, the hot, and the sweating.

Cold period.—This begins with yawning, listlessness, and a peculiar sensation in the back, or in the ends of the fingers, which are not easily described. The patient is then visited by a sense of horror; a tremor of the muscles, with a chattering of the teeth; a sensation of extreme cold, which commences at the extremities and travels to the trunk; it appears sometimes to be deeply seated, even to the bones; a pain in the joints, back and head; difficulty of breathing; pellucid urine, and a quick, small, weak, contracted pulse, which can scarcely be numbered on account of the tremor. It lasts from a few min-

utes to three or four hours, and even longer, when reaction is not effected; death frequently happens in this stage.

During the action of this period, the patient betrays a constant disposition to yawn; the skin becomes so powerfully contracted that the finger rings will drop from the hand; the mind becomes unsteady, irritable, taciturn, and morose; the muscular tremor is sometimes so violent as to resemble a paroxysm of convulsions; these rigors, as they are usually called, when long continued, leave the patient greatly exhausted. A dry cough, deep sighing, and a feeling of stricture across the chest are usually present.

Dr. Eberle says, that in debilitated persons a violent fit of rigors often induce a complete state of stupor or coma; but in the south, we have seen these symptoms in the athletic. In this period, the thirst is urgent, and although cold water is desired, the patient fears to take a second draught, because he found the first to increase the paroxysm. The chill is sometimes so slight as to escape the cognition of the patient; the finger nails, more than any other part, expose the fact.

As this period becomes merged in the next, a sensible and a very unpleasant mixture of heat and cold travels over the patient. About the time of this change, nausea and vomiting supervene, but sometimes earlier, and continue until the hot stage is formed.

Hot period—Pyrexia.—As the last stage passes off, an intolerable heat succeeds. The arteries are dilated, and strike forcibly against the finger; the respiration is strong and free. The head aches and the patient frequently becomes delirious, or we should have said, perhaps, that when the patient is of a dense fibrous constitution, he is very apt to become delirious. An intense thirst is complained of, and a frequent demand is made for cold water; the tongue is white; the urine is high colored; much heat is complained of about the præcordia, and sometimes there is a swelling of the epigastrium. The blood, when drawn, is more dense than usual, red on the surface but dark colored within, with only a small portion of serum, and less cohesive than in health.

When this stage is being introduced, a warm and pleasant glow is felt about the face and temples, and from this the whole surface becomes hot; nevertheless, at this early stage of the period, if a limb be moved, sudden sensations of cold will be felt running, as it were, in divided rays over the surface. But presently all traces of the former period have passed off; the skin becomes distended with blood, the cheeks are flushed, and the eyes become brilliant. Dr. Wood says that Fordyce found the temperature of the system to be 105° by the thermometer, and Mackintosh, he says, has known it to be as high as 110° in Great Britain, and 112° in warm climates. There is

no desire for food, and sometimes nausea and vomiting are present.

As to duration, this stage is as various as the preceding one, but it is generally more lengthened, extending sometimes to eighteen hours. The febrile action is frequently greater than in continued or remittent fever. The cerebral suffering in this period differs from that of the cold stage in this, it is more deeply seated and generally more severe. At the commencement of this period in children, convulsions not unfrequently occur.

Sweating period.—The patient, at length, feeling as though he was scorched to a crisp, is relieved by a copious sweat all over his body. All the symptoms now remit, and the perspiration, after continuing three or four hours, but sometimes much longer, entirely disappears. The urine deposits a lateritious sediment; the patient falls to sleep, and an intermission succeeds.

Apyrexia.—This period embraces that time which transpires between the close of the sweating stage and the commencement of the forming one of the next paroxysm. During this period, the patient, though without fever, is not usually entirely free from some sensations of ill-health. He feels as though he had been sick; he moves about as though he suffered from languor, and possibly he has some obtuse pain in the back and loins; his appetite is fastidious, and his complexion is more or less sallow. But, on the contrary, it frequently happens that he feels entirely clear of disease and has an appetite that is even less fastidious than in health.

We shall now make an extract from Dr. Wood, for the purpose of showing how little is understood of the nature and *modus operandi* of fever. We select him, not because he is the most vulnerable, but because he is probably the most esteemed authority in this country. He says:

“It has frequently been stated by writers that each stage of the paroxysm, subsequent to the chill, is the immediate effect of the preceding stage; in other words, that the cold stage produces the hot; and the hot the sweating; but this is scarcely probable, at least with regard to the first two stages, which bear no proportion to each other—a protracted and severe chill being frequently followed by less fever than a very slight one, and the fever sometimes occurring without any preceding chill whatever. The probability is, that while the depression attendant upon the cold stage is naturally followed by some degree of febrile reaction, as a necessary consequence, the morbid cause, *whatever it may be*, is capable of producing the hot stage by a direct influence.”

We would like to know what sort of action this “direct influence” is. We are utterly unable to conceive how any

one can pursue a sound practice in fever, whose views of its pathology are so beclouded—the fact is, they do not, and can not.

We do not expect to explain this matter to the satisfaction of the lovers of the mysterious, but we are of the opinion that it can be done for those who seek only for the truth, and love it the more for its simplicity. Then, first, what is fever? We have answered this question several times, but it will do no harm to do so again. It is vital action accumulated for the removal of obstructions from the system. Now, a proper application of the law contained in this answer, will enlighten the whole subject.

When the vital force is great, and the obstruction is comparatively trifling, is it not obvious that the former may act upon the latter without the patient or his physician having any other cognizance of the fact than is rendered apparent by the sequence, which is fever? It is not characteristic of a great man to make much fuss over a little thing, then why should we expect such a vital force to make a great fuss—to shake the patient almost to pieces, to overcome a little obstruction? A little obstruction may occasion a more lasting fever than a great one, because the concentration of vital force will be less—its energies will not be thoroughly aroused.

When the vital force is compared with the amount of the obstruction, if feeble, the chill will be thorough and protracted, and in the event that it is overcome, it has been effected by a thorough concentration of the force; and whenever successful, there will be no great excess to become manifested in the form of fever.

In the former case, though the fever may be considerably protracted, the patient will recover without foreign aid; but, in the latter, two or three paroxysms may so exhaust the vital force that death may result in the next struggle—simply because it is overpowered by the obstruction. In the latter case, furthermore, when successful, almost as soon as the cold stage has passed, the sweating or secreting stage will be introduced, simply because the force did not and could not rise much above the secretory condition of the system.

Upon this subject, we beg leave to refer Drs. Wood, Eberle, Cheyne, Watson, and the whole Allopathic school, to the "Thesis" of Freeman Franklin, M. D., of the Eclectic Medical Institute, of Cincinnati, which will be found in the April number of the Eclectic Medical Journal of that school for 1853. We say unhesitatingly, and under a full sense of our responsibility, that he has conveyed, in four or five pages, more sound and correct views of the pathology of fever, than can now be found in every Old-School book extant. The

young gentleman merits this compliment, and the Old School the rebuke.

As a difference of latitude, of topographical condition, of constitution, and of modes of life, produce great departures from the general history we have given of this form of fever, it becomes proper that we should notice a few of them. The principal of these peculiarities Dr. Eberle has reduced to the following: 1, the *inflammatory*; 2, the *congestive*; 3, the *gastric*; 4, the *malignant*; to which we add: 5, the *masked intermittents*.

1. *Inflammatory Intermittents*.—According to the same author, Richter is of the opinion that this form of intermittent fever occurs more frequently during the winter and spring, and that it occurs more frequently in quotidians than in tertians, and in the latter more than quartans. Those who constitute our first class are the most frequent subjects of it.

It is usually introduced with rigors, and in the hot stage the temperature of the surface rises high and the pulse beats with fullness and strength. But the diagnostic symptom of this form is the fact, that no matter how thoroughly marked the sweating stage may have been, a state of pyrexia will still continue to the next paroxysm. Much of the violence of the hot stage, to be sure, has subsided—the pulse has lost its fulness, but much of its quickness and tenseness remains; the thirst, too, is reduced, but still the patient demands drink, and still complains of some pain in the extremities and back. The discontented and fretful condition of the patient indicates the existence of much irritability in the system. Richter, says Dr. Eberle, teaches that this variety of intermittents is but seldom attended with gastric disturbances.

2. *Congestive intermittents*.—This form of the disease does not occur often in this climate, nor very frequently even in the south. Those who become the subjects of it are of the third class, and who, in the north, would probably die of phthisis pulmonalis.

For the reason that we have before given, the cold stage is greatly protracted, and all the symptoms indicate a highly congested condition of all the viscera, such as pain in the head, a sense of weight and oppression in the chest, fainting, vertigo, a feeble pulse; beside these symptoms of the cold stage, the hot is never clearly developed—it does not present a single feature of this period as it usually obtains. It develops itself very slowly—the skin scarcely becomes warm, the respiratory function continues oppressed, the countenance continues to appear anxious, and the pulse, though frequent, is small and tense—and probably the only heat complained of appears to be internal, and but seldom demanding drink.

3. *Gastric intermittents*.—This variety, as might be inferred from its name, is attended by much gastro-enteritic irritation, from the redundancy of bile, a hyper-secretion of which is going on in the liver; the skin wears a jaundiced complexion; the urine is charged with bilious matter; nausea and vomiting of bilious matter is common; the tongue appears to the taste to be bitter; much pain is complained of in the forehead. As the sequelæ of this form of disease, we may anticipate much visceral derangement, as a cachectic condition of the system, and induration of the liver and spleen.

This form of disease usually appears in the fall, and has, in the opinion of the writer, no necessary connection with the, so-called, miasmata. He has witnessed it only in those who had been exposed to the preceding summer's sun, whereby the cutaneous system became, not only deranged, but greatly debilitated, and through a failure of its function, the disease resulted.

4. *Malignant intermittent—Congestive chill*.—Dr. Eberle says, that, according to Alibert, this form is of frequent occurrence in the south, and is always of a dangerous character. It is characterized, he says, "by a copious and fetid perspiration in the third stage, together with colliquative hemorrhages from various parts of the body, sometimes petechiæ and other marks of malignity. They run their course with great rapidity, death usually taking place in the third paroxysm."

The writer has seen, and has had to contend with, many cases of this form of disease, and he has never known it to occur except in the first class of persons—the strong and more or less athletic. The first and second paroxysms are, so far as he has observed, uniformly mild, and the patient, during the intervals, will employ himself as usual; but the third paroxysm is characterized by the above-narrated symptoms. Alibert says that death takes place in the third paroxysm;—to this remark we will add, and it always happens in the cold stage.

It is thought to be, and if neglected or inefficiently treated it certainly is, a dangerous disease. The writer thinks it hardly probable that one per cent. of unaided patients recover; nor has he ever known one to recover in the Old School practice—and yet, in the main, its practice was founded upon correct indications, but it wanted energy. He is decidedly of the opinion that there is no form of disease more manageable than this; and this opinion he founds upon his own uniform success. And why should it not be? It never assails the feeble, nor those of a broken-down constitution; but those who are otherwise sound, and so strong as to require only proper and prompt aid to effect a reaction.

5. *Masked intermittents*.—Under this head, it is useless for

us to do more than to apprise the young physician that among those forms of disease which, in the normal mode of manifesting themselves, are familiar to every practitioner, do frequently obtrude themselves upon our notice in a masked form; and what is still more important to be known, is the fact that they will yield to no other means of cure than those which are applicable to the regularly-formed intermittent fever. In other words, it appears that the disease or obstruction which generally results in what is known as intermittent, does, in consequence of local or some general variety of infirmity, appear in that form of disease in which the said infirmity would most likely manifest itself. As an illustration of our meaning, we remark a tooth is so decayed as to expose its nerve, which by aching indicates the usual morbid action of the system, laboring under the usual cause of intermittent fever, the reactive power expends itself in the form of odontalgia. This, we opine, is the true explanation of the fact, just as gastric derangement is manifested in the same manner, and for the other reason; that masked intermittents can be cured only as pure intermittent is.

Of the masked form of intermittent fever, Dr. Brown (En. Prac. Med.) has given a catalogue, from which the following are extracted—but without his appended authorities: Pleuritis, carditis, peritonitis, ophthalmia, swelling of the head, urticaria, scarlatina, rheumatism, gout, epistaxis, odontalgia, cephalalgia, encephalitis, meningitis, gastroenteritis, dysentery, asthma, hysteria, epilepsy, convulsions, blindness, dumbness, sneezing, eructation, leucorrhœa; but neuralgia, in some form, is the most frequent that appears in this disguise.

PROGNOSIS.—Simple or uncomplicated intermittent fever can not, directly, be regarded as a dangerous form of disease, but when long continued it is apt to lay the foundation of a premature death. The feet and legs become edematous, the spleen enlarged, the digestive function greatly impaired, and finally dropsy terminates the case. But the practice of irregular habits, and the use of trashy and innutritious food, have as much to do in these results as the disease—indeed, we have seen but few who possessed a good constitution, and lived obediently to the organic laws, who ever had the disease.

When the disease assails those who possess a highly cephalic constitution, passive congestion of the brain, or apoplexy, may be so far anticipated, as to place us on our guard. Prof. Eberle says: “In general much less danger is to be apprehended from this disease in the young and vigorous, than in persons of feeble, nervous, and depraved habits of body.” We should think so, as we have never seen the disease, except in the malignant form, in persons of that character. He further states: “Delirium seldom occurs in intermittents, and

when it does happen, it must be viewed as unfavorable; and even more so than mere coma." We do not believe one word of this. Dense and fibrous constitutions very rarely have febrile action without delirium; and yet they have the least danger from this disease. The writer, during five years, had more than twenty paroxysms, and delirium attended them all; and yet he never had a threatening one. We have seen much of the disease, and would greatly prefer to see delirium than coma.

Before closing, it is proper to remark, that tertians are more manageable than quotidians, and they again much more so than quartans. It is our opinion that simple intermittent fever is always founded on debility—and hence remittents are converted into intermittents by bleeding and purging.

CAUSES.—In treating of the causes of fever in general, that which pertained to this particular form was so far discussed, as to leave us but little to say at present. Our readers have, no doubt, learned by this time, that we have no more faith in the doctrine of *kaino-miasmata* or malaria, than a man of common sense might be supposed to have in the so-called "spirit rappings." It is, we believe, a settled principle in philosophy, that like causes will produce like results—hence like results should be referred to like causes. Bear this in mind, while we attend to Prof. Eberle. He says: "I have seen one instance, in a delicate child, where a distinctly-formed ague was manifestly produced by intestinal irritation, from too free an indulgence in irritating articles of food. An interesting case is related by Mr. Earl, in which a regular intermittent was produced by the irritation of a small piece of dead bone in an old wound, and which was at once arrested on removing the irritating substance." He further states, "Richter observes, that worms and other causes of intestinal irritation have been known to produce intermitting fever."

What a wonderful similitude there must be between irritating articles of food, worms, and a small piece of dead bone, and *kaino-miasmata*!

Let us notice him a little further. "Intermittents are never more prevalent than when the days are very warm, and the evenings and mornings are cool and damp."

We are disposed to think that such mornings and evenings, acting upon a skin that had become exhausted from previous long exposure to a hot sun, would be sufficient to produce an adequate obstruction to call forth an action of the system.

PROXIMATE CAUSE.—Upon this subject, we have had nothing but speculation, and hence a little more can do no great mischief—therefore, we claim a brief indulgence.

Periodicity is a law of the animal economy, and therefore we shall attempt no explanation of it, but we may elucidate

it. A man sleeps to-day at a given hour, and therefore, on the morrow he will desire to sleep again, unless his mind shall have been pre-excited. This is our experience. Application: A man has a febrile paroxysm to-day, why, therefore, should not his system require one at the same hour on the following day?

But there is another view that may be taken of this subject. Obstruction to the performance of some essential function has been produced; vital force accumulates and acts for its removal; but the force accumulated in the viscera and large vessels was at the expense of the surface and extremities, and consequently in them, the calorific function is greatly weakened for a time, or suspended, and a chill is the result. Next, the force acts, and the action is called fever, and in this action it is expended, and depuration is the result; but the obstruction is not all removed, and therefore, as soon as preparation can be effected for another paroxysm, it returns; and thus a habit is finally established which, sooner or later, under favorable circumstances, will spontaneously leave the system. Connected with the subject there are many minor phenomena, for which we have no explanation.

INDICATIONS.—As intermittent fever results from obstructions in debilitated constitutions, the indications must be to assist the vital force to remove the former and to strengthen the latter.

TREATMENT.—At the commencement of this disease, an emetic must be administered, if possible, just previous to the return of the cold stage, which may be followed by a purgative, if the patient's strength will admit; and in very severe attacks, this course must be continued daily for several days in succession. After the action of the first emetic, anti-periodics must be given, and continued through the whole course of the disease, without regard to any of its particular stages. The anti-periodics which have proved most efficacious in our hands, are Quinia, Prussiate of Iron, Hydrastin, and Cornin, in the following combination:

R. Sulphate of Quinia,
Fero-cyanuret of Iron,
Hydrastin,
Cornin, *aa* xv grs. Mix.

Divide this into ten powders, of which one may be given every hour, until the disease is arrested. If much influence upon the head should ensue, the dose of Quinine may be lessened or altogether omitted. If catharsis is required during the treatment, a combination of Podophyllin and Leptandrin, or the Compound Pill of Podophyllin, will be preferable to all other cathartics, as they act both by freeing the bowels and unloading the congested liver.

In cases where there is much gastric irritability, accompanied with nausea and vomiting, and an inability to retain anything upon the stomach, sinapisms applied to the epigastric region, hot applications to the feet, with Lupulin or small portions of Opium carefully administered, will, if persevered in, overcome the difficulty; when the curative treatment may be pursued.

The Extract of *Leptandra Virginica* and *Salicacæ* have been employed by one of the authors with much success in this disease (*Ecce. Med. Jour.*, 1852, vol. iv, p. 295). We extract his remarks upon the use of the latter:

"*Of the Salicacæ.*—We make no pretensions to botany, and therefore, we can not designate the two varieties of the tree of which we shall treat. They are, beyond doubt, indigenous. Our acquaintance with them was contracted in Arkansas: one is common to the Arkansas and Mississippi rivers, and probably to many other streams; it grows into a very tall tree, and, in some instances, obtains a diameter of two feet. Its twigs or small branches are very brittle at their junction with the stalk, of a darkish yellow, not bright, like a foreign variety, which is common about Cincinnati. The tree is straight, and grows near the edge of the river, and frequently forms dense groves of small growth on the sand beaches.

"The other variety, in Arkansas, grows scrubby, about the size of a peach tree, on stony branches, where, in the summer season, there is scarcely any moisture.

"Knowing that some varieties of the willow had been used as an anti-periodic, we were, during our first season in the State, forced, for the want of a better, to use the extract of the bark of the tall willow first above described. The result was so satisfactory that we sought no other agent while in the vicinage of the river.

"During our third summer in the State, we attended a very large political gathering of the people of the valley of the Arkansas river, upon which occasion we were called upon for a political speech; but as the opportunity did not suit our politics, we gave them one upon the cure of intermittent fever, which prevailed very extensively among them.

"We instructed them to fill a ten gallon kettle with the twigs and bark of the Arkansas willow, and boil them about thirty minutes, then re-fill the kettle with fresh material, and thus proceed until they shall have a kettle full of a strong decoction; then strain it and boil it down to a syrup; and then, to avoid burning it, evaporate it in the sun to a proper consistency to make into from four to eight-grain pills.

"In the use of these pills, we instructed them to resort to no variety of preparatory medication; but to commence six or seven hours before an expected paroxysm, and give two of these

pills every hour, until the chill came, or its time for appearing should have passed.

"In the following winter, we again visited that portion of the State, when a Mr. Barnets informed us that the pills he made cured seventy-four or five cases; that another gentleman, a few miles from him, had cured more than fifty; and that, in all, he supposed one hundred and sixty cases, in his acquaintance, had been cured by our address.

"In our practice, we never knew the chill to follow these instructions, when thoroughly carried out.

"It is not our opinion that this willow has much, if any, of the astringent principle; these pills had, more frequently than otherwise, an aperient effect.

"Being, upon another occasion, in the southern and mountainous portion of the State, we were called to an obstinate case of intermittent fever, and had no medicine to combat it with. The Leptandrin was not known to exist about there, nor did the tall willow, but the scrubby willow abounded. Supposing, as others have stated, that all the willows possessed similar qualities, we had an extract made of it, and administered it as above taught. It produced no observable tonic or anti-periodic virtue, but so obstinately locked up the bowels as to do more mischief than could have been compensated by a destruction of the chill. It is a powerful astringent, and appears to be as destitute of tonic power as the other is of astringent.

"There is, in this vicinity (Cincinnati), a willow precisely resembling the Arkansas, except in size; that their virtues are the same, we can not answer. Among the forest timber, there is another similitude equally striking: the buckeye of this country is a tree, in that, it is a shrub; in all other respects the resemblance is perfect, even to the effects upon the cattle which eat it."

The Tincture of Gelsemium is extensively used in this disease by many of our physicians, and, as far as we can learn, with much success; our use of it, however, has been limited, yet, as far as tested, we are much pleased with its effect.

Although we have stated that an emetic will be advisable at the commencement of an attack of intermittent fever, yet it will rarely be required after the first week of its existence.

Notwithstanding the immense amount of matter from time to time presented to the profession for the treatment of this disease, we have found the above simple plan fully sufficient to meet the most obstinate and severe cases generally met with.

SPECIES II.—*Remittent Fever—Bilious Fever—Bilious Remittent Fever.*

In order that the reader may not be mislead by names when reading other works on fever, we deem it proper to state, that this form has received many, in consideration of the locations in which it has fatally prevailed, or from the local circumstances which are supposed to give origin to it, viz: miasmatic fever; marsh fever; country fever; river fever; lake fever; Bengal fever; African fever; Walcheren fever; Mediterranean fever; but it is, by the American readers, most generally understood by the names of bilious fever, bilious remittent fever, and remittent fever; yet, even these names are objectionable, because all of them are sometimes just as applicable to other varieties of febrile action. Nevertheless, the term *remittent* conveys more nearly the idea we wish to convey than any other by which it has been designated, inasmuch as it really holds somewhat of an intermediate position between the continued and intermittent forms of the disease; and it may be taken to represent, in respect to violence or grade, a mean degree between intermittent and yellow fever.

Prof. Caldwell used to maintain that the principal difference between these three forms of disease consisted, mainly or altogether, in the violence or concentration of the cause; and this opinion, we believe, is considerably prevalent in the profession. To those who have not studied closely the difference in human constitutions, this doctrine may appear almost or quite unexceptionable; but our long acquaintance with the latter subject causes us to entertain some doubt, more especially as it affects the intermittent and remittent forms. We are much disposed to regard these forms as such varieties only as may result from differences of constitution.

Under the head of remittent fever, writers embrace two widely-differing varieties: the *sthenic* and the *asthenic*. In the former, all the vital functions are vigorously performed; while, in the latter, the vital system was either organically feeble or had become so by previous disease, or debilitating habits. When treating of intermittent fever, we found that the malignant variety never obtained with those of a feeble constitution. In this form, then, the *sthenic* and *asthenic* can only be reconciled as members of the same form, upon the principle that intermittent and remittent forms of fever are one and the same, modified by telluric, atmospheric, and constitutional differences.

So far as our observation has extended, and it has not been very limited, intermittent fever is produced by the action of a humid atmosphere upon feeble constitutions—the humidity preventing a proper cutaneous action; and remittent fever results from a less humid and a highly-heated state of the atmosphere acting upon highly-vital constitutions; hence, in new countries, as we have seen, remittent fever may prevail in the towns, and intermittent in the country where forest and marsh abound. This explains also, why it is that intermittent fever prevails in the south at the river's edge, and remittent on the top of the bluff, where ponds and marshes do not exist.

From such facts as we have here introduced, it would seem that strong vital force is best adapted to an unexciting atmosphere, and that a feeble one does best in an exciting or electric one. We may further add, that we have always found malignant intermittent to obtain under remittent influences.*

Dr. Joseph Brown (Cy. Prac. Med.), treating of this subject, says: "Remittent is the endemial of warm climates, especially of those of which the soil is marshy; but it is to be met with in the more temperate regions of the earth, and is not unfrequently observed in our own country, especially in seasons of *universal heat*, and in those parts of it where under ordinary temperatures agues are prevalent." This observation, as far as it goes, supports directly the opinions we have advanced.

Dr. Wood says: "Remittent fever has the same types as the intermittent. The most frequent is the quotidian, with a paroxysm occurring at about the same time every day. The tertian, with its every-other-day paroxysm, is not uncommon. The quartan is very rare. Next, perhaps, to the quotidian, is the double tertian, having a daily paroxysm, but that of one

*The writer was early induced to observe closely the relations of autumnal forms of disease to their causes, because he rejected the doctrine of malarious poison when a student of medicine, and under the voice, too, of its champion advocates, Professors Caldwell, Chapman, and Hossack. He will not assert that the doctrine of malaria is false, but that he believes it to be, and expresses this conviction for the purpose of inducing his readers to observe and investigate the matter for themselves. He would have them, as he has endeavored to do, to take nothing for granted that has not been demonstrated, unless it be self-evident. This course is indispensable, if we would escape imposition, for the most flagrant errors have received the sanction of truth, by the universal consent of mankind. Illustration: Mineral coal is of *vegetable* origin!

day differing from the next, and the alternate paroxysms resembling each other both in character and in time of occurrence."

The phenomena of the forming stage in this form of fever are, in general, such as we have before detailed under the head of "Order I;" but sometimes this period is remarkably brief, for the second or cold stage, as indicated by slight chills, is frequently the first manifestation of the disease. For a time more or less definite, these chills alternate with flashes of heat, until finally the latter acquires an entire ascendancy—the febrile action is fully established, and with a highly increased condition of many of the usual prodromal symptoms; such as, pain in the back, head, and extremities. The pain of acute rheumatism is not more severe than that of the extremities frequently is. The tongue is covered with fur; a jaundiced complexion spreads over the eyes; nausea, which is sometimes attended with a vomiting of bilious matter; the right hypochondriac and epigastric regions labor under a sensation of weight or tension; the urine is scanty and charged with bile; the skin is generally dry and hot; the respiration is oppressed; the pulse is but seldom hard or tense, but generally full and frequent, although not to such an extent as to exceed 116 or 120 beats per minute; the appetite is not gone, but there is usually a loathing of food; thirst is not a constant though a frequent symptom. It is common for the eyes to be suffused and the face flushed.

These symptoms, from having continued from six to eight or ten hours, relax, and the stage of remission commences. About the neck and face some moisture appears, which may or may not extend itself over the whole surface—giving so much relief that the patient soon becomes lost in a quiet and refreshing sleep. All of the febrile symptoms have abated, but a state of apyrexia does not supervene. The duration of the abatement or remission is very indefinite—extending from two to twenty hours, depending upon the existing quotidian or tertian type, as the case may be, when another febrile paroxysm supervenes, commencing in many instances as the previous one did, with chilly sensations, and subsiding in perspiration. This alternation of exacerbation and remission continues, with a gradual increase of the former and diminution of the latter, until the zenith of the disease is attained. But it is proper to remark, that the remission, instead of being attended with perspiration, will frequently give no other evidence of its existence than some abatement of the symptoms.

In mild cases, the tongue is disposed to be moist throughout the disease; but, with the usual violence of the disease, it becomes dry and frequently brown, or even blackish, particularly

about the surface. In most cases, the epigastrium becomes particularly tender to pressure; in some instances, gastric oppression is complained of, and in others, a sensation of burning pain which is very distressing. During these symptoms there is apt to be much gastric irritability.

One of the distinguishing symptoms of this form of disease is yellowishness of the skin and of the white of the eyes, and though not always present in the beginning of the malady, it rarely refuses to make its appearance by the fifth day; its tint of color sometimes becomes very intense, even simulating that of yellow fever, and not unfrequently quite uniform over the system; it has even been known, in some instances, to be so secreted upon the surface as to stain white articles coming in contact with it. If bilious diarrhea supervenes, this peculiar secretion of the skin will not be produced.

At the beginning of the disease, nausea and possibly vomiting may be among the symptoms; they are rarely absent, however, when at its height. The ejections are, generally, in the latter case of a yellowish, greenish, grass-green, bluish, or brownish color, and of a bitter taste. The force of the symptoms are not unfrequently reduced by nausea.

The bowels are not always constipated, but that they are, may be assumed as a general fact; and the fecal excretions, though sometimes anti-bilious, are generally the contrary, and probably mixed with some shade of green or black. In the advanced stage, a bilious diarrhea sometimes supervenes, and occasionally it is present at the beginning, and when nausea happens to be present, also, the introduction of the disease has somewhat the appearance of a cholera morbus. As most generally obtains in fever, the urine is small in quantity, high colored and turbid, and in an advanced condition of the disease, it is usually of a brownish color. As might be expected, there is some increase of it during the remissions.

There is, perhaps, no symptom that is more uniformly present than headache, and along with it there is generally one or more of the following symptoms: vertigo, intolerance of light or sound, roaring or singing in the ears, redness of the conjunctiva, flushing of the face, delirium, drowsiness, tetanic spasms, paralytic symptoms, wakefulness, hiccough, etc.

Finally, the reader is not expected to meet, in observations upon this form of disease, with all the symptoms we have enumerated in any case; and he should remember, furthermore, that there is perhaps no other form of fever that presents so many modifications of type, grade, and variety. We have therefore, only attempted to represent that character which may be drawn by generalizing a majority of its visitations.

Dr. Joseph Brown says: "Recovery may be expected if the pulse becomes more full and expansive, the heat more equal-

ized over the surface, the pain of the head and epigastrium less distressing, and if the countenance, though still flushed, loses the purple hue which it previously possessed. A favorable inference may be drawn, too, from the remissions becoming more distinct, the conjunctivæ being less vascular, the bowels more obedient to the action of purgatives, the restoration of a more abundant urinary secretion, and the appearance of bile in the matters vomited. The mean duration of the disease, in cases of recovery is about fourteen days."

Dr. Wood makes the average duration to be about the same period, but very properly adds, that it sometimes terminates as early as the *fifth* or *seventh*, and often about the ninth or eleventh; but sometimes it is protracted to twenty-eight days.

The writer never had a case to extend beyond nine days, and frequently they terminated on the seventh; yet, in the practice of his neighbors, he has known cases to continue twenty-seven days and never less than twenty. He alludes to those cases only to which he was called in consultation that had passed into the typhoid character—which never happened in his practice. He does not believe that the disease has a prescribed course to run. An energetic and proper practice will never suffer a case to run fourteen days.

Remittent fever is considerably modified by the season of the year in which it may appear. When it appears in the spring, it is apt to produce thoracic symptoms of a dangerous character; when in the summer, the brain and the nervous system become the most vulnerable, and in the autumn, the abdominal viscera will suffer the most.

Dr. J. Brown treats of one symptom which, it seems, has been noticed only by Dr. R. Jackson and himself; it is what we would denominate a suicidal monomania, instead of general delirium. It seems to have puzzled him very much, more particularly as *post mortem* examinations revealed nothing! If, in such cases, he had examined the parietal ridge, immediately behind the coronal suture, he would have found that part largely developed. Monomania of any kind, in all febrile cases, depends upon special mental powers in a high state of endowment or excitement. He speaks also of some who, while in the fever, predicted that they would die—and die they always did, but the secret cause and fulfillment of the prediction he could not divine. If he had phrenologically examined them, he would have found a large endowment of caution, and a small one of hope. Even in cases of general delirium, it is not uncommon for some particular idea to predominate. As to the circumstance of their dying, there is nothing strange; but it would be strange for men to recover from under a constantly abiding impression of fear, so common and

natural to such an organization, more especially when submitted to, as an inevitable doom.

In the course of this fever, those of its subjects who possess a part so feeble that it can not resist invasion, inflammation is apt to be induced, and upon this circumstance, rather than upon the inherent character of the fever, does its fatality depend. The most vulnerable parts are usually the stomach, the spleen, the lungs and the brain. When inflammation supervenes, the treatment will be found under the head of inflammation of these organs, respectively.

It sometimes happens that this fever presents only a typhous character from the beginning. This may depend upon an inherently feeble constitution, but we are of the opinion that it originates more frequently in those who have become enfeebled by previous disease or vicious habits. Bilious fever, of the ordinary character frequently degenerates into the typhous through neglect, but more frequently through mal-practice—the very common anti-phlogistic. When the disease is of this character, the treatment will be found under the head of typhous fever.

It frequently happens that individual parts share a greater portion of the diseased action than others, and that, too, without the existence of inflammation; but it is proper to add, that such local manifestations of disease should render us vigilant lest inflammation should supervene. Upon these local determinations, varieties of this fever have been established; hence we have gastric-bilious fever, hepatic-bilious fever, and upon the same principle we have as many as there are important parts in the system; nevertheless, these local modifications should always command our earliest attention.

CONVALESCENCE.—We have but rarely witnessed a case of convalescence which was not more retarded by the mal-practice that had been instituted, than by any pre-existing peculiarity of the fever or of the constitution, and the principal difficulty we always found to be referable to mercury or the lancet. We have known the latter to be used to reduce the fever to an intermitting form—one that essentially depends upon debility. Such practitioners must act upon the principle that it is more easy to remove the system from the disease, than it is to remove the disease from it. In consequence of the use of the lancet and of mercury, the convalescence is not only greatly protracted, but attended with much suffering.

When the case is of a mild grade, the convalescence is rapid and altogether favorable, and such is the case in almost all of its modifications when the treatment has been judicious. The troubles that are usually classed under this head consist frequently of the manifestations of that disease which was instituted to remove the pre-existing one. In originally frail con-

stitutions, this stage may be attended more or less with copious night-sweats, imperfect digestion, constipation, enlarged spleen, and perverted action of the liver.

CAUSES.—Upon this subject we have perhaps said enough, unless we had something more certainly true than has yet been divulged. The profession generally attribute it to *mia ma*, but many think it may be produced by heat and moisture. In answer to this hypothesis, Dr. Wood inquires why it does not occur among sailors at sea, and in our cities. We answer: At sea, the temperature is never so afflicting, and in our cities, in consequence of its hard, paved, and shaded condition, there is comparatively but little moisture. But, in this matter, all have overlooked an important feature—the electricity evolved by evaporation. One square foot of black, moist, and heated soil will throw off more vapor and evolve more electricity than a square rod at sea or in the city. Here, we are satisfied, lies concealed the secret. Under the head of intermittent fever, we delivered our opinion as to the cause of that and also of this form of fever.

DIAGNOSIS.—When we shall have treated of the two following species, the reader will have but little use for a special treatise on the distinguishing differences between this and other forms; and yet, there is, perhaps, no single symptom, taken separately, that may not occur in other forms of fever. To judge of it at all, we must view it as a whole. The most marked symptoms of this form are: 1st, its paroxysmal character; but this is even more perfectly possessed by the intermittent. 2d. Gastric irritability; this symptom attends this form more habitually than it usually does others, but others sometimes equally manifest it. 3d. Constant pain in the head; this is like the preceding. 4th. Bilious phenomena; although these are more generally present, yet the same is true occasionally in other forms; hence all effort to divide febrile forms of disease into well defined species, must fail. Fever, therefore, is essentially the same in all of its modifications.

PROGNOSIS.—Under this head, we have the same difficulties to contend with. In taking a general view of bilious fever, we are prepared to declare it to be favorable. Under a prompt and judicious treatment, we consider it (all circumstances considered) a less fatal and a more manageable form than the intermittent, simply for the reason, that in it there is more vital force, generally, to contend with the disease. It should never be considered as dangerous, except when it invades a diseased or broken down constitution.

INDICATIONS.—In this form of fever, these are very simple, but require an energy which shall be in harmony with the vital force; they consist in equalizing the circulation and the

nervous irritability of the system, to remove obstructions and all sources of irritation, and to promote depuration.

TREATMENT.—At the commencement of the febrile stage of this form of fever, or during the first day or two of its attack, one of the best agents that can be given is an emetic, which not only removes the morbid matters accumulated in the stomach, but, by giving a shock to the nervous system generally, causes more or less profuse diaphoresis, mitigating the subsequent severity of the disease, and not unfrequently breaking it up at once.

The emetic operation should be followed by a cathartic, for the purpose of removing congestion of the portal circle, augmenting the secretory action of the liver, and unloading the bowels of fecal and bilious accumulations. To effect this, we know of no better agents than a combination of Podophyllin, Leptandrin and Jalapin, which may be given in doses and at intervals suited to the urgency of the case.

After the action of the cathartic had ceased, should there still remain pain in various parts of the system, nausea, or vomiting, or rapid pulse and high fever, active diaphoresis must be produced and maintained for a few hours. To effect this, the spirit vapor-bath is the best method; and may be given while the patient is in bed; or the Compound Tincture of Virginia Snakeroot may be administered in suitable doses, and repeated at short intervals, until the patient perspires freely. The Gelsemin or the Tinct. of Gelseminum may also be used.

If these symptoms, however, should not be present, or only slightly exist, the practitioner may commence at once with anti-periodics, and continue them through the whole course of the disease. The agents of this class and combinations which we prefer, have already been given under the treatment of the preceding forms of fever.

For the determination to the head, which is almost always present in this disease, the same course must be pursued as already named in the treatment of continued fever.

Hiccough may be relieved by Aqua Ammonia, Compound Spirits of Lavender, Musk, or other anti-spasmodics. Restlessness, wakefulness, and similar symptoms may be relieved by Compound Powder of Ipecac, and Opium, Sulphuric Ether, or infusions of Valerian, Sculleap, or Black Cohosh.

Convulsions, which usually take place with children, may be treated by the warm bath, sinapisms to the extremities, and the internal exhibition of a dose or two of the Tincture of Lobelia and Capsicum.

The patient may drink freely of cold water, and the diet should be light and fluid, with proper ventilation of the room and changing of linen. If acidulous draughts are craved, such as lemonade, tamarind water, or currant jelly, apple-sauce, and

similar digestible preparations of acid fruits, they should be permitted. During convalescence, the diet must be more nourishing, but strictly of a digestible character.

SPECIES III.—*Congestive Fever—Pernicious Fever—Pernicious Remittent—Pernicious Intermittent.*

Dr. Wood thinks the name “pernicious fever” to be less exceptionable than any other that has been selected to represent this form of fever. This, in the abstract, may be true, but the one we have adopted has become so familiar from long use, particularly to our western and south-western people, to whom this work will be measurably confined, that we prefer it.

If congestive fever was not so signally distinguished in the popular mind from all others, we should have treated of it as a mere variety of the preceding, which, in truth, is the fact. Indeed, we may say that all the forms comprised in this genus are but varieties—modifications, and they should be so considered.

Dr. Wood says, that he does not apply the epithet pernicious to all fatal or dangerous cases of either intermittent or remittent fever, but to those “in which there is great and sudden prostration or depravation of the nervous power, or to use a customary phrase, in which the *innervation* is extremely and most dangerously defective or deranged.”

This manner of defining his meaning amounts to about nothing, because it applies with as much justice to those forms of fever which are malignant or pernicious in consequence of a depraved constitution. In such cases, there is prostration—a want of innervation. Our idea of what he intended to define is about this: A pernicious fever is one in which the obstruction exceeds the resisting power of the vital force—one which will readily recover when the force is so aided as to overcome or remove the obstructions. By this state of the system is commonly understood a congestion from the magnitude of the invasion, and not from any previous depravity of the constitution; and one that is entirely independent of any local inflammation.

The importance of this distinction is very considerable, because, in the first, though the present congestion may be relieved, the patient will probably die by reason of constitutional depravity; but, in the second, if it be overcome—if reaction be established, the patient is saved—dissolution and recovery depend, respectively, upon the turning of a single point and this point, to a malignant extent, rarely recurs.

Between this form and the one of malignant fever, there is one striking similitude, it never occurs in a depraved or feeble

constitution—but in the strong; and there is also an equally striking difference; the congestive chill is confined to the usually miasmatic season of the year; but this has been known to commence in the spring and to prevail through the winter, though it generally obtains in the same season with the preceding.

It is found to be difficult to state what the type of this fever will be, until after two or three days—the period of doubt and uncertainty to both the patient and physician—that of congestion, or struggle or effort to overcome the obstruction. If this period be survived the type may be intermittent, remittent, or continued. But if the congestive period be procrastinated for two or three days, it may for this time appear as an intermittent or a remittent; and, subsequently, instead of either of these types, it may appear, as before stated, in the continued form.

Observations from various parts of the country seem to have determined, beyond doubt, that there are two leading modifications or varieties of this malady; in one the cerebro-spinal or animal system seems to be almost exclusively invaded; and in the other, the ganglionic. Whatever may be the character of the disease as subsequently developed, it usually begins, as do the miasmatic fevers in general, and betrays no pernicious phenomena until the arrival of the second or third paroxysm; or, as before intimated, at the close of the prodromal stage a highly-congested condition of the system may become manifest.

If the fever is to be of that modification which more especially invades the cerebro-spinal system, the first indication will probably be drowsiness, which will be followed by more or less of stupor. The patient's attention becomes greatly impaired and his memory unfaithful. When asked a question, he may be so far aroused by it as to attempt an answer, but will probably stop before he has concluded, and possibly in the middle of a word.

This cerebral condition may, and not unfrequently does, pass into a complete coma, and from an impossibility to arouse him, he will die in it. The circulation appears labored, and though the pulse indicates fullness, the rapidity is frequently even less than the normal standard; the respiration is stertorous. It is said that epileptic convulsions do sometimes attend this fever, and also tetanic spasms and difficult deglutition.

These symptoms sometimes occur in the first paroxysm, but more frequently they are procrastinated to the second, or even the third. This form is consistent from the start, for though the first paroxysm may be light, yet comatose symptoms will prevail more or less. If the patient shall rally from the first severe paroxysm, which is generally the second, the following

intermission will be attended by its usual symptoms; indeed, he may be exempt from all symptoms of disease; and thus go on to recovery; but it is unsafe to indulge, without the greatest precautionary means, such a hope, for most frequently he will continue drowsy and stupid until the next paroxysm, which usually proves fatal.

The *Medical and Surgical Journal* of New Orleans, vol. iii, p. 733, contains an article from Dr. Bowling, of Montgomery, Ala., upon the subject of this fever, as he observed it. He states that the approach of the paroxysm, after a few febrile exacerbations, was attended by spasms which closely resembled an attack of tetanus, that, with exacerbations and remissions of fever, continued with a corresponding degree of violence. He further states, that after the occurrence of the spasm, the patient in no instance lived longer than five days, and most frequently, if not arrested, death supervened at an earlier period. But the disease, as he saw it, differed from its usual character in this: it was attended with some intellectual capacity.

When it invades the ganglionic system, the sight of one patient, in the forming stage, will so impress his general pathognomy upon the memory of the physician that he will never forget it. Language can convey no adequate idea of it. His expression is one of alarm, of horror, and impending dissolution; his features are shrunk, shriveled, and apparently reduced within two thirds of their former compass; his eyes are sunken in their sockets, and the skin is of a bloodless paleness. The extremities and the whole surface are cold, but the patient does not appear to be conscious of it, and a clammy perspiration covers the surface. Sometimes, as an exception to this general coldness, the chest and the abdomen are much heated. The tongue is occasionally pale and cold, and the stomach greatly oppressed; and yet, with these symptoms, the patient complains of an intense internal heat, and of unquenchable thirst.

In many instances, the stomach is particularly irritable, and all ingesta are rejected, and with it sometimes a little bile or muco-serous fluid, and possibly a little blood; and, as in the case of congestive chill, the bowels may labor under a sero-sanguineous hemorrhage, the discharges being frequent and greatly prostrating. The respiration is difficult or labored, being quick and panting; the pulse is feeble, or it may be corded, and so frequent as to number 160 beats per minute. Those who have practiced in the cholera, but have not seen this form of fever, would, upon seeing it, suppose it to be the former—it resembles it in very many particulars. The preceding symptoms, with modifications, may continue two or three days, and unless relieved will terminate in death; or, if not too oppressive, they may be followed by a remission, or even an inter-

mission, when the same symptoms will be repeated with increased and fatal violence when the proper hour is brought about by the revolution of its type.

In a majority of cases, however, these dreadful symptoms may continue but a few hours, before they are replaced by a more favorable set, as returning warmth to the surface and extremities—by a stronger and fuller pulse; in fine, by a regularly-formed hot stage, which as to mildness will be in the ratio of the preceding severity.

CAUSE.—About this we absolutely know nothing; but a few facts have been observed in relation to it. It has been observed in some places, that when bilious fever prevailed on the table-lands, this form took place or was produced in still lower situations near the river's edge. It has been further observed, that when remittents and intermittents have prevailed, this variety did not.

Dr. Wood is of the opinion that it is a miasmatic fever, but it appears to us rather difficult to reconcile this idea with the prevalence of the disease through the winter, as is sometimes the case.

NATURE.—Dr. Wood asks: "What is it that imparts its peculiar character to the pernicious fever? Can it be inflammation?" He appears to come to the conclusion that it is not—and we think this conclusion is very correct. He then asks: "Is congestion the source of danger? I can not think so."

When we consider the cold and shriveled condition of the extremities and surface generally, we can not doubt the presence of congestion even to an alarming extent. We care not whether the danger be attributed to congestion, or to the obstruction, for the removal of which the congestion was the first preparatory step, because, if we equalize the circulation, and thereby re-establish depuration, we shall remove the disease.*

* In the treatment of this disease, the indications are, very generally, by the Allopathic faculty, properly judged of, but their therapeutics are defective in energy. The writer has witnessed the death of many patients from this and other congestive forms of disease, simply through a want of energy. To melt a pig of iron it is necessary to apply heat, but to expect to melt it in a pile of shavings or straw, would betray a remarkable degree of folly—not more, however, than most physicians manifest with regard to congestive forms of disease. To place around or about the patient a few bottles of hot water, and to administer to him a little toddy and a little Ammonia, is equivalent to melting a pig of iron with straw.

Through a similar want of energy, he has known death to result from coma. Through a fear of public opinion, physicians frequently omit to discharge their own convictions of duty—such men should not practice medicine. An illustration of this may not be out of place. A professional acquaintance of the writer had a case of coma, and after having, with commendable zeal, tried all the means to arouse his patient that had been

TREATMENT.—The most reliable treatment is artificial heat, together with counter-irritation, most energetically adopted. The most active and powerful means must be applied to procure a determination to the surface, and thereby relieve the oppressed condition of the nervous system, as well as the congestion of the internal viscera; for instance, if possible, the warm or vapor-bath may be used, hot foot-bath, sinapisms to the whole surface of the inferior extremities and spinal column, having previously applied powerful, stimulating liniments with considerable friction.

As to internal treatment, Quinine may be administered in very large doses, as soon as possible, without regard to any particular stage or period of the disease; and its administration should be continued, at short intervals, until a decidedly normal impression is effected.

We have found the tincture of Gelseminum given in connection with Quinine, in these cases, to be without a parallel in efficacy; it must be given, however, until its peculiar effects are induced, which may be known by an inability of the patient to raise his eyelids, or muscles generally, although perfectly conscious of all around him; together with a sense of increased temperature of the surface, a return of the skin to its normal condition, and a greater degree of regularity and softness of the pulse. In addition to this, the various preparations of Xanthoxylum, the bayberry and ginger in the form of hot decoctions, may be used freely, and often repeated.

In many instances, diarrhea is present, and the discharges are sometimes hemorrhagic, for which Geranin may be given in large doses in conjunction with the above remedies; but if, on the contrary, there should be constipation, active doses of Podophyllin must be given; and, in either case, as soon as the condition of the patient will permit, a combination of Podophyllin and Leptandrin should be administered, at short intervals, for the purpose of restoring the bilious secretions to their normal standard.

When stupor or coma is present, ice-cold applications must be made to the head, in connection with the above counter-irritating means to the spine and extremities, and stimulating enema may be frequently employed, together with active

recommended by authorities, but to no effect, he sent for counsel. The consulting physician suggested *actual* cautery to several inches square of the surface on the lumbar region of the spine. The attending physician refused to accede to it, on the ground that the patient would die anyhow, and if such a measure were used, the people would say that they had killed him. The consulting physician then requested the privilege, upon his own responsibility, which, after consultation with the mother, was granted. Hence the cautery, in the shape of a hot shovel, was applied—and the patient saved. In this instance, but for the energy and decision which every physician should possess, the patient would have been lost.

doses of Podophyllin, if the patient can be made to swallow them.

Cold water may be drank freely by the patient, and even in large quantities if craved.

After the disease has been subdued, the treatment during convalescence must be similar to that already recommended in other forms of fever.

SPECIES IV.—*Yellow Fever—Typhus Ictericus—Remitting Yellow Fever—Bulam Fever.*

This is another form of fever which seems to be produced by a longer continuance or some modification of that cause from which the other forms of this genus result. In its individual character it presents many and considerable extremes of variety; so much is this the case, that a physician introduced to it for the first time, would be strongly induced to suspect, in a number of hospital cases, that he had almost as many independent forms of fever to contend with. Although the several patients may have been assailed at the same time, yet no two of them will have the same symptoms, in the same order, or manifested in the same manner. Much of this variety must be referred to inherent differences of constitution; and some of it, no doubt, is referable to the magnitude of the cause of the lesion inflicted.

The prodromal stage or period partakes largely of those peculiarities which attend the other forms of fever. In the period of chill, rigors very rarely obtain, and in some instances there is not even an appreciable sensation of cold or chill—the hot stage being gradually merged out of the incipient, which is greatly distinguished by the severity of the pain in the back and extremities. The stage of excitement is characterized by a dry and hot skin, frequent pulse, a flushed face, red and surfeited eyes, and a hurried respiration. The stomach is disturbed by nausea, and possibly by some vomiting, as both are not unfrequent attendants. Deglutition is frequently rendered difficult by the soreness of the fauces, and the tongue wears a white fur in conjunction with considerable moisture. Gastric disturbance is often present from the beginning, but generally it does not appear until after the lapse of twelve or eighteen hours, when it becomes exceedingly troublesome—attended with a sense of burning pain, tension, oppression, and soreness upon pressure—all ingesta are retained, while, singular to remark, its own morbid contents are retained. The bowels, as is common to febrile action, are constipated; flatulence is often troublesome, and the desire for cold drinks is very imperious.

Writers generally inform us that the tongue usually, if not always, wears a pasty coat, but Dr. Gillkrist says, that a per-

fectly clean tongue is not an unfrequent attendant upon very severe cases. From our acquaintance with fever in general, we are disposed to regard the latter as a more unfavorable symptom than the former. He adds, that the most characteristic symptom of this fever is a pasty tongue, with the tip and edges red. In young and vigorous subjects, the skin may, and often does, have a pungent heat, but we are not to suppose that this is a more dangerous symptom than even a much lower temperature. In the latter case, the skin has a peculiar condition—one that can best be represented by the idea of compression or impaction, which is by no means to be regarded as favorable. The patient is apt to suffer much from his incapacity to obtain rest—he is restless—incessantly tossing his head and limbs about—unable to sleep—he is deeply distressed—draws deep inspirations; in fine, all of his manifestations indicate a state of congestion—one of imperfect reaction.

The nervous symptoms provoked by this fever are surpassed by no other class, in relation to the suffering they produce. The severe pains in the back and extremities with which the affection was introduced, increase and continue even through the stage of excitement; pains in the head and eyes usually attend, are generally severe, and, like the former, continue through this stage, and with it, all of them so increase in violence as to cause the patient to moan and even to scream. The mental functions are frequently deranged; sometimes it amounts only to confusion, but at other times it is manifested by maniacal violence.

As the disease advances, the irritability of the stomach becomes so excessive that it is perfectly useless to attempt to do anything through it in the way of medication. Dr. Gillkrist (Cyc. Prac. Med.) says: "Our experience during two epidemics, one in the West Indies, and of 1828 at Gibraltar, by no means bears out the statement of others as to the *bilious* appearance of what is vomited in the progress of this fever; after having paid the closest attention possible to this point, we must, on the contrary, state that with the exception of the black-vomit stage, and at the very commencement of the attack, what is thrown up consists of the ingesta and a glairy fluid. Bile is also usually absent on an inspection of the stools and urine." But he goes on to remark, that it should be remembered that we are treating of a disease which is so much like spasmodic cholera as to furnish exceptions to almost every rule that can be drawn from its diversified manifestations.

Dr. Eberle says, that after the first ejections, bile in abundance is frequently brought up, which varies in color from yellow to dark green, and often of so acid a quality as to excoriate the fauces.

The stage of excitement, with occasional rigors, may vary, says Gillkrist, in its duration from forty-eight to seventy-two hours; the pulse begins to give way, after having, up to this time, been full, rapid, and firm; the eyes lose their brilliancy, and the patient, in most instances, becomes exceedingly feeble—too much so to sit up without aid. The skin becomes cooler and moister, the respiration calm, and the stomach measurably easy; the severity of the headache and other pains have departed, and the patient believes himself to be convalescent, and such may even be the fact; but in the majority of cases the struggle is yet to be encountered.

During this calm, however, we are not without indications of the existing probability. If the worst is yet to be expected, the epigastric tenderness, upon pressure, will be found to have increased, and instead of the flushed face and red conjunctiva, we will find an orange yellow extending from the face to the chest, and possibly it may invade the entire surface; the urine has also a yellowish tinge, though in a deep quantity it may appear brown, and the pulse has fallen below the normal standard. This intermission or remission, as the case may be, may continue only a few hours, or it may continue twenty-four. But, be it long or short, it is succeeded by pain in the stomach and a burning sensation, and to these succeed a most distressing vomiting; the ejections are flocculent, and the desire of cool drinks urgent, and which, as soon as swallowed, are rejected with a force that leaves no doubt as to the extreme irritability of the stomach. These symptoms, having continued from twelve to thirty-six hours, introduce the last or third stage.

The vomiting now becomes almost incessant, and the matter thrown up resembles coffee-grounds suspended in a glairy liquid, which is both dark and ropy; the pulse sinks in force, volume, and frequency; the tongue is dark, approaching black in color; the burning sensation in the stomach is extremely distressing; the extremities are cold and clammy; green or black discharges from the bowels now ensue; and then follow hiccough, hemorrhages, delirium, coma, convulsions, and death.

This is perhaps the most general course of this fever, when fatal; but it must be remembered, that it is frequently still more overwhelming; the patient sometimes falls at the seizure as though felled by a blow; and in some it commences with furious delirium. In such cases, it is common for the disease to terminate fatally at the close of the third day; but even in these cases, dissolution is preceded by an acrid or burning sensation in the stomach and extending to the esophagus. The temperature of the surface and extremities falls rapidly; the

features shrink; the urine is absent; distressing singultus comes on, and finally black vomit.

But it is much more common for the disease to be protracted to the fifth, sixth, or seventh day, and sometimes fatal cases are continued to the twelfth or even the fifteenth day. The yellowishness of the skin frequently appears as early as the third day, but more frequently it does not appear until a more advanced period, and in a few instances, not at all. Hemorrhages from the nose, gums, decayed teeth, and every other orifice of the body are apt to appear about the fourth day.

The profession are generally of the opinion at this time that the matter of black vomit consists of coagulated particles of blood suspended in gastric mucus; it is insipid, whereas the black vomit of bilious or typhous fever, when it occurs, is bitter.

CAUSES.—The time has been when this subject was much discussed, but it has measurably passed. During the studentship of the writer, it was strenuously maintained by some to be contagious. Prof. Hossaëk told the writer that if he would hear him through his course on the subject, he would convince him that it was contagious; he did so attend, but was not convinced. Prof. Caldwell maintained that it was not contagious—and in this we are disposed to believe that he was correct—but he also maintained that it was produced by a peculiar poison, which he denominated malarious or miasmatic. This may be true, but the writer has never believed it. It is, and has been, his opinion that the various forms of fever embraced by this genus result from various combinations of heat, humidity, and electricity; but, however this may be, the weight of the profession has concurred in the opinion that yellow fever is not contagious, and in this we think an important point has been gained.

We do not intend to enter into any discussion of the subject in this place, but will introduce some facts which may possibly aid others to think for themselves upon the subject:

1. Yellow fever does not prevail in the cold season.
2. It does not require a higher temperature than 79° or 80° of Fahrenheit.
3. It is arrested by cold, heavy rains and storms.
4. It appears simultaneously, and is intermixed with bilious remittents.—*Eberle*.
5. Dr. Ramsey states, that during the prevalence of the yellow fever at Charleston, in South Carolina, in the year 1804, “neglected intermittents frequently terminated in yellow fever.”—*Eberle*.
6. Dr. Rush also states, in relation to the yellow fever at Philadelphia, in 1802, “Intermittents, the mild remittent, the inflammatory, bilious, and the malignant fever, have, in many instances, all run into each other.”—*Eberle*.

7. Dr. Caldwell, in speaking of the yellow fever of Philadelphia, in 1803, states: "As the fever receded from the low ground and malignant atmosphere of Water street, it became more and more manageable, until its evanescent shades in Second street were, in many instances, much lighter than the common remittent of the country."

8. In the city of Baltimore, "the bilious or vomiting fever, in its ordinary form, prevailed in that town and continued until it was gradually lost in the severer form of yellow fever as the season advanced."—(*Danage*) *Eberle*.

9. "In some epidemics, females have remained wonderfully exempt; this was the case during a terrific epidemic at Dominique and Martinique, in 1801."—*Gillkrist*.

10. In 1811, at Xeres, females "suffered in a particular manner."—*Gillkrist*.

11. Negroes are known to be, generally, very insusceptible to the cause of this disease.

We have in another place shown why it is that women and children are measurably exempt from any form of fever to which the other sex is peculiarly liable. This fact can not be explained by assuming any modification of the poison, if such it be. We have also explained why it is that negroes are but little liable to the southern fevers.

Yellow fever is sometimes produced under circumstances entirely incompatible with the idea of a malarious poison. Dr. Gillkrist, upon this subject, says: "In fairness, we can not think that, like many other places which may be mentioned where yellow fever is known from time to time to appear, it can not be admitted that Gibraltar furnishes sources from which *malaria*, in the usual sense of that word, arises, sufficient to account for the appearance of a malignant fever; neither can we concede to authorities of great respectability, that either there or in various other places the solution of the question is to be found in a crowded population, the filth of the town, or the state of the sewers.

"If we consider the soil and elevation, it must be admitted that here, too, no satisfactory conclusion can be drawn; for if we find evidence, especially in the West Indies, and on the American continent, of the influence of a marshy soil, on several occasions, this does not hold good in other instances; and in those countries, as well as in Spain, many places might be mentioned, where elevation, soil, etc., would seem to guarantee immunity, but where, nevertheless, the disease occasionally prevails to a devastating extent."

If we had space to enter fully into this subject, it would be discovered that all the facts which favor the doctrine of malarious poison, equally favor our own views, and that those

which are fatal to that hypothesis, are entirely compatible with ours.

If yellow fever be caused by miasm, it is supposed that it must in some way be associated with the decomposition of animal matter, because the disease is confined to cities, armies, ships, etc. The disease is never known in New Orleans when the river keeps full during the hot season; and when it becomes low, there is exposed an immense mud-bar through the whole extent of the city's front, in which was deposited, during the business season, an immense quantity of animal matter. Under such circumstances, is it not possible that the atmosphere may generate a species of animalculæ which may be inhaled, and thus produce the disease? Or, in lieu of this, may it not cause their generation in the human system? Dr. Rhees, of Philadelphia, states: "When the matters fresh thrown from the stomach were examined, the animalculæ were alive and in constant motion; but that no such phenomena could be detected in autumnal or bilious fevers." This circumstance demonstrates the existence of a very great difference between the two varieties of fever.

DIAGNOSIS.—All fevers, in their initiatory stage, have so many features in common, that it is difficult to distinguish this from other febrile forms under two or three days, and yet there are signs which may guide to a very probable conclusion, such as the turbid conjunctiva, the great severity of the pain in the loins and inferior extremities, and, when the malady has further advanced, the great epigastric tenderness and the gastric irritability, the regular continuance of the fever for two or three days, and then succeeded by an abatement—intermission or remission, the yellowishness of the eyes and skin, and finally the appearance of the black vomit. If yellow fever prevails at the time, or if the other usually attending circumstances of the disease be present, the preceding symptoms can scarcely leave a reasonable doubt as to the character of the disease.

PROGNOSIS.—This has hitherto most generally proved to be a very fatal form of disease, having its fatality sometimes to run as high as ninety-five per centum. We feel quite sure that such a fatality is, in a great measure, due to the prevailing ignorance of its pathology and requisite therapeutics.

It is now a well settled fact, that the disease is attended with a much more extensive mortality when it first appears than at any subsequent period—that its fatality declines from the beginning of the epidemic to its close; and from this circumstance it appears to be generally inferred, that the cause, whatever it may be, is more concentrated or capable of inflicting disease than it becomes to be at a later period. We regard this inference as being true only to a small extent, if at all. Our conclusion is,

that the cause, when it becomes first introduced, cuts down all of the most vulnerable to its influence, and as the vulnerability diminishes, so do its fatality and the number of its victims. Dr. Wood says: "Perhaps the general average of deaths from yellow fever, prevailing epidemically, may be stated at one-third."

Among the unfavorable symptoms, Dr. Wood cites the following: Excrutiating pains in the forehead, back, and limbs; great frequency and fullness of the pulse, a gaseous state of it, or its entire absence at the wrist; a blood-shot appearance of the conjunctiva, and a bronzed or mahogany-color of the skin; a short and violent febrile stage; coma or convulsions; a slow respiration with deep sighs; hiccough; excessive restlessness, and a disposition to get out of the bed and walk; an unnatural apathy, or an expression of dogged indifference in the advanced stages; a voracious appetite; suppression of urine; an universal hemorrhagic tendency, with petecchiæ; and finally, the occurrence of the black vomit. This last symptom is regarded as almost necessarily fatal, but occasional recoveries are mentioned by authorities as having taken place after its appearance.

INDICATIONS.—Whatever the cause may be can matter but little when we know the absolute condition of the system, and that this is one of a high order of congestion there can be entertained no reasonable doubt. The indications then, must be to overcome all cutaneous stricture and stimulate its surface, and by revulsives to produce a centrifugal action in the circulation—in other words, our duty is to equalize the circulation, and to establish and maintain depuration.

TREATMENT.—In the early stage of the disease, should there be no irritability of the stomach, an emetic must be given, to be followed by an active cathartic, for which the Compound Powder of Jalap is admirably adapted; after the operation of the cathartic, the Compound Powder of Ipecacuanha and Opium, combined with equal parts of Quinine, may be given every hour or two in three or four-grain doses; and the bowels must be kept open daily, in every case, by small portions of Podophyllin and Leptandrin, frequently administered; but in cases of irritability of the stomach, in which everything is rejected from it, Mustard must be applied over the epigastric region, and energetic counter-irritation to the spinal column and inferior extremities, together with active cathartic injections, combined with pulverized Lobelia or a portion of Tincture of Lobelia and Capsicum. The patient may also eat ice in proper quantities, which will usually be found to assist in overcoming the gastric irritability.

The surface of the body, in all instances of this disease, and especially when gastric irritability is present, should be closely

attended to, the whole surface should very frequently be bathed with a cold alkaline wash, which should be continued until the temperature is much diminished, and renewed whenever there is an increase of fever. In other respects, pursue the same course as laid down for congestive fever. Cold water may be allowed whenever the patient craves it, but should it be almost immediately rejected, small portions of ice should be substituted.

SPECIES V.—*Infantile Remittent Fever.*

We have concluded not to separate this form of fever from the general subject, as nothing could be gained by so doing.

This species has been divided into three varieties: the acute, slow, and low—and the differences between them are sufficiently considerable to justify the distinction.

VARIETY I.—*Acute Infantile Remittent.*

This form is distinguished by pain in the head and abdomen, slimy stools, little thirst, loss of appetite, drowsy exacerbations, and wakeful remissions; it begins, usually by several days, with marked symptoms of indisposition; the patient's color is changed; its expression contra-indicates health; its breath is offensive; it picks its nose and lips; has a dry cough, anorexia, swollen abdomen, and flatulence; it grates its teeth, moans and starts in its sleep; its urine is turbid, and upon cooling it deposits a whitish sediment; and its bowels may be either loose or costive. In the midst of these symptoms a chill is produced, which is soon followed by fever; but sometimes its beginning is sudden, and no introductory stage becomes apparent.

Of the paroxysms there are frequently three in twenty-four hours—one in the forenoon, one in the afternoon, and one at night, which is usually the most intense. It is hot and of course restless at night, and the diurnal exacerbations soon succeed. When the fever is severe, the remissions are short, and frequently not more than observable; it is attended by troublesome flatulence, an increased cough, a hurried respiration, and occasionally by nausea and vomiting. As the fever progresses, the exacerbations increase, and all of its attendant symptoms become aggravated, so that the pulse runs from 140 to 160.

At length, all the symptoms during the remissions abate; it begins to rest composedly; the pulse is reduced to 120 or 130 per minute; it is more wakeful, gives more attention to passing events, and is not unfrequently playful. In the ex-

acerbations and remissions the skin is usually dry, and, when an exception occurs, the moisture is usually confined to the head, breast, or palms of the hands. As yet, neither food nor drink is desired, and when either is taken it is usually rejected as soon as swallowed. The urine has become limpid and of a high color, and the stools never have a normal appearance in smell, color, or consistence; worms are sometimes found in the stools and vomitings, and they spontaneously appear through the anus or the mouth; the exacerbations become milder and more abridged as the pyrexia declines; moisture generally appears upon the surface; the pulse falls; the appetite and refreshing sleep return; a copious sediment deposits from the urine, and the alvine excretions have become more healthy. The complaint continues from one to three weeks, and occasionally longer.

VARIETY II.—*Slow Infantile Remittent.*

There is a variety of infantile remittent which makes its assault more insidiously and imperceptibly, and by way of distinction it is called the slow variety. In this form the breath is offensive, the appetite is fastidious, the abdomen frequently enlarged, and the flesh is gradually wasted; it has but one exacerbation, which comes on in the evening, continues until morning, and is then succeeded by a profuse perspiration; hectic flushings are frequent through the day, the skin is dry and harsh, the pulse about 140 in the exacerbation, and about 100 in the remission. The patient is rarely so much indisposed as to be confined to bed; nevertheless, it is not inclined to be active or playful, but is indolent, listless, and complains of an aching of the limbs when motion is attempted. It is often considerably disposed to doze both day and night; to pick its nose; the urine is of a deep orange color; the thirst is but little; the appetite is absent; the tongue is white and moist, and the stools, as in the former, or acute variety, are unnatural. At the commencement of a favorable termination, all the symptoms abate, and health and cheerfulness return. The continuance of this form of disease may be of two or three months.

VARIETY III.—*Low Infantile Remittent.*

This form resembles very closely, for the first week, the acute variety, except that it begins, pretty uniformly, in a sudden manner, and afflicts the brain more severely, often to the production of delirium. Its peculiar character now commences by an exhibition of indifference to all that surrounds it; it calls for nothing, and yet refuses neither food nor drink when presented to it; the superior extremities are usually,

during its waking hours, kept in constant motion, while its inferior continue in a fixed posture. When the disease has attained to its most oppressive stage, such is the debility that the patient loses the power of speech, and its jaws occasionally become fixed and immovable. During the exacerbations it slumbers much; the eyes are dull, inattentive, and flushed when the stage begins; the tongue, teeth, and lips are covered with a blackish fur; the facial expression betrays much distress, and during the remissions much restlessness becomes apparent by the constant motion of its arms. Before a discharge of feces or flatus, it becomes restless, and, though sensible, the cystic and alvine excretions are passed involuntarily. In the remission, the pulse is about 100, but in the exacerbation it rises to 120. When the exacerbations become shorter and milder, a favorable turn is supposed to have commenced, and if so, the drowsiness will be reduced, the eyes will become clearer, the expression more placid, the pulse more calm, the tongue more clean, and the appetite improved; it gradually becomes to betray some fretfulness, the speech and voice return, but the urine is still passed involuntarily—weakness is the only matter of complaint, and this is finally overcome. This form may continue six weeks, or even longer.

CAUSES.—Upon this subject there has existed considerable contrariety of opinion. At one time it was attributed by many to the presence of worms; so much so was this the opinion that Hoffman was greatly surprised when he did not find worms in connection with this disease. At that day the disease was familiarly called “worm fever.” Something like a hundred years since, an Edinburgh physician questioned the doctrine, and showed that worms were not always the cause of this fever. Dr. Butter, in 1782, in his work on this fever, attributed the disease to debility, derangement, etc., of the digestive organs. He was of the opinion, however, that worms were of advantage to the system—and even Dr. Rush was much inclined to the same view of the subject. At the present time, the profession is generally of the opinion that Butter’s views, in the main, are correct. In France, it is considered as a species of gastro-enteritis. It occurs both sporadically and epidemically. In the latter form, it is produced, probably, in organically liable constitutions, by some of the usual causes of fever. Dr. Sims reports one epidemic of it as prevailing simultaneously with a low grade of nervous fever among adults (Cy. Prac. Med.).

DIAGNOSIS.—Dr. Sims, above cited, was of the opinion that it was impossible to distinguish this fever from hydrocephalus in patients under five years of age; but Pemberton is of a contrary opinion. He thinks, that the screaming of the patient during sleep, strabismus, tossing of the hands over the

head, intolerance of light, and the disturbed condition of the intellectual faculties, which characterize hydrocephalus, will distinguish it from this form of fever, in which there is no strabismus, no derangement of the intellectual faculties, and but very seldom any screaming during sleep or any intolerance of light. Furthermore, says Dr. Golis, hydrocephalus is attended by no distinct stages, its pulse never falls below the normal standard, and it never extends from three to six weeks.

PROGNOSIS.—Although this is a troublesome and tedious disease, it can not be thought very liable to any unfavorable termination, more particularly under a judicious practice. In fatal cases the intestines have been found greatly distended and the mesenteric glands somewhat enlarged, but neither inflammation nor effusion has been detected in the abdominal cavity. The fact that this fever is somewhat liable to pass into hydrocephalus, constitutes its most dangerous feature. When such a tendency exists, it is usually indicated by sickness and vomiting in the beginning.

TREATMENT.—At the commencement of the disease, when the stomach is loaded with undigested food, or before gastric irritation has taken place, a light emetic will usually be found advantageous. This should be followed by an active purgative, as the Compound Powder of Jalap, which is probably the best agent that can be used, often exerting a beneficial influence over the disease sufficient either to put an end to it at once, or greatly mitigate its subsequent severity.

After the bowels have been freely acted upon, should the fever continue, small doses of Leptandrin, with or without Podo-phyllin, or if there be gastric acidity, Leptandrin combined with the Compound Powder of Rhubarb, or Syrup of Rhubarb and Potassa, should be administered in doses sufficient to cause one, but not to exceed two daily evacuations from the bowels; and in cases of excessive irritability of the stomach, in which these agents are rejected, active cathartic injections must be used.

Gastric irritability may be overcome by mild mucilaginous draughts of a diuretic nature, as Marsh-Mallows, to be aided by external applications, as sinapisms to the epigastric region and spinal column. Spasmodic action will usually yield to the Tincture of Lobelia and Capsicum, aided by warm baths, especially if it amounts to actual convulsions. Probably the Tincture of Gelsemium might be of service when these symptoms are present. When great determination to the head is manifest, cooling lotions must be frequently applied, together with warmth and counter-irritation to the inferior extremities and spinal column.

As soon as the stomach can retain medicine, anti-periodics must be given continuously every few hours, until the solution

of the disease; the following is perhaps equal, if not superior, to any other for this purpose:

R. Powder of Ipecac. and Opium, ʒss,
S. Quinine, ʒj. Mix.

Divide into ten powders, and, to a child two years old, give one powder every four or five hours.

As soon as convalescence takes place, the mild bitter-tonics, as Hydrastin, Cornin, etc., must be given, and their use continued, until the strength and appetite are restored. The diet must be very light, and increased as the restoration to health advances. Oftentimes, when this disease is very obstinate, a removal to the country or change of climate will effect an immediate beneficial result.

TYPHOID AND TYPHOUS FEVERS.

We here present to our readers the investigations of Dr. W. Jenner, in relation to these two febrile forms of disease, which we regard as an excellent analysis, and of much value to the practitioner:

On Typhoid and Typhous Fevers. An attempt to Determine the Question of the Identity or Non-Identity, by an Analysis of the Symptoms, and of the Appearances found after Death, in Sixty-Six Fatal Cases of Continued Fever, observed in the London Fever Hospital, from January, 1847, to February, 1849. By W. Jenner, M. D., Professor of Pathological Anatomy in University College, London.—This is the title of an extremely interesting paper which has been published in successive numbers of the Monthly Journal of Medical Science, commencing in the number for April of last year, and concluded in the number for April of the present year. It constitutes one of the most important contributions to the history of continued fever that has yet been made, presenting, as it does, the carefully recorded histories of a very large number of cases, admirably arranged and analyzed, by a highly competent and reliable observer.—*Amer. Jour. Med. Sci.*, for 1850.

“*Age.*—Typhoid fever was limited, in the cases here considered, to persons under 40 years of age; nearly one-third of the forty-three cases of typhous were more than 50 years of age.

“*Mode of attack.*—As a general rule, the attack of typhoid fever commenced more insidiously than that of typhous fever.

This observation, like all others in this paper, applies, of course, only to fatal cases.

“*Duration*.—The average duration of the fatal cases of typhoid fever was 22 days. Of the fatal cases of typhous fever, 14 days. Half the cases of typhoid fever survived to the 20th day of the disease. Not a single case of typhous fever survived the 20th day of disease.

“*Eruption*.—The difference in the appearance of the eruption in the two diseases was as great as it well could be, considering that both were of a reddish hue.

“*Miliary vesicles and sudamina*.—These vesicles were present in an equal proportion of the cases of both diseases under 40 years of age. But in no cases of typhous fever, more than 40 years of age, were they detected.

“Subsequent experience leads me to believe that miliary vesicles are rarely seen on individuals more than 40 years of age; and very rarely, indeed, if ever, on patients more than 50 years old. I have, during the last year—*i. e.*, since my attention was directed to this point—seen these bodies on no one of the many patients more than 50 years of age, laboring under various diseases, that have come under my observation.

“*Expression, manner, hue of face, etc.*—As the rule, in the cases of typhoid fever here analyzed, the expression was much less indicative of prostration, and more anxious, than in cases of the typhous fever. In the former disease, the complexion was tolerably clear, and the flush, when present, was of a bright-pinkish color, limited to one or both cheeks, and often distinctly circumscribed. In typhous fever, on the contrary, the complexion was thick and muddy, the flush of the face uniform, and of a dusky-red color.

“*Headache* was a constant symptom in all the cases of typhoid and typhous fevers; but it disappeared about the 10th or 12th day in the latter, and not until the termination of the second, or the middle of the third week, in the former.

“*Delirium* commenced in three only of ten cases of typhoid fever before the 14th day; while it began in fourteen out of fifteen cases of typhous fever before the 14th day. As a rule, the delirium was decidedly more active in typhoid than in typhous.

“*Somnolence*.—In eight out of nine cases of typhoid fever, somnolence commenced after the 14th day of disease. In seventeen out of eighteen cases of typhous, before the termination of the second week.

“*Coma-vigil*.—One-fifth of the cases of typhous fever experienced coma-vigil; not a single case of typhoid fever experienced that condition.

“*Spasmodic movements* were nearly equally frequent in the two diseases.

“*Retention of urine and involuntary discharge of urine and stools* occurred with equal frequency in the two diseases; but at a much earlier date in typhous than in typhoid fever.

“*Loss of muscular power.*—Little more than a fourth of the patients attacked with typhoid fever kept their beds entirely before the seventh day of disease. All the patients affected with typhous, whose cases are here considered, took altogether to their beds before the seventh day of the disease.

“The prostration was rarely so extreme in the cases of typhoid fever as in those of typhous fever. Extreme prostration, when it did occur in typhoid fever, was not observed until from the 14th to the 30th day, while in a large majority of the cases of typhous fever it was marked between the 9th and 12th day of disease.

“*Epistaxis* was present in five of fifteen cases of typhoid fever—in not one of twenty-three cases of typhous fever.

“*Hearing* was equally and similarly affected in the two diseases.

“*Eyes.*—The conjunctivæ were *very much* more constantly and intensely injected in the cases of typhous than in those of typhoid fever; the pupils were absolutely larger than natural in a majority of the cases of the latter disease, while these were abnormally contracted in a large majority of the cases of the former affection.

“*Tongue.*—Although individual cases of the two forms of disease may have closely resembled each other in the appearance of the tongue, yet, taking the whole of either group of cases, this organ presented a singularly different aspect in the one from what it did in the other. It was much more frequently moist throughout the disease in typhoid than in typhous fever. When dry, it was often red, glazed, and fissured, in the former; but rarely so in the latter.

“Again, in typhoid fever, when the tongue was brown, its hue was much less deep—it was of a yellowish, instead of a blackish brown. The small, dry tongue, with red tip and edges, smooth, pale, brownish-yellow fur, fissured—the surface seen between the fissures being red—may be considered differentially as a diagnostic sign of typhoid fever. One only of the twenty patients affected with the typhoid fever, but eight of the forty patients laboring under typhous fever, were unable to obtrude the tongue when bidden.*

“*Intestinal hemorrhage* occurred in one-third of the patients

*This clearly indicates the difference in the amount of prostration in the two diseases.

laboring under typhoid fever—in none of those suffering from typhous fever.*

“The other abdominal symptoms and signs need no recapitulation.

“*Appetite and thirst.*—No difference in the two diseases.

“*Pulse.*—The frequency of the pulse fluctuated much more, from day to day, in the case of typhoid than in those of typhous fever.

“*Cough and physical chest-signs.*—Sonorous rale was very much more frequently present in the cases of typhoid than in those of typhous fever—*i. e.*, it was present in eleven out of twelve cases of the former, and in seven only of twenty-one cases of the latter. Dullness of the most depending part of the chest, from intense congestion of the lung, was observed in nine cases of typhous fever—in no case of typhoid fever.

“*Sloughing* appeared to be nearly equally frequent in the two diseases.

“*Erysipelas* occurred in seven of the twenty-three—*i. e.*, in nearly a third of the cases of typhoid fever; and in two only of the forty-three cases of typhous fever—*i. e.*, less than one-twentieth of them.

“*Discoloration of the walls of the abdomen, and of the skin covering the larger veins,* was much more frequently present in those dead from typhous than typhoid fever.

“*Emaciation* had made greater progress in the typhoid than in the typhous subjects.

“*Spots.*—The spots observed during the progress of the cases of typhous fever continued after death; no trace of the spots visible during life could be detected after death from typhoid fever.

“*Head.*—After typhoid fever, the pia mater and arachnoid separated from the convolutions with abnormal facility in one only of nine cases examined with reference to the point. The vessels of the pia mater were abnormally filled with blood in one-third of the cases, but intensely congested in one only of fifteen cases. After typhous fever, the pia mater and arachnoid separated with abnormal facility in nine of eleven cases of which notes on this point were made. The vessels of the pia mater were congested in nearly half, and intensely congested in one-fifth, of the whole of the cases; while the cerebral substance itself was abnormally congested in half.

*I may here remark, that in one case only of typhoid fever, received into the London Fever Hospital during the last three years, has blood passed from the bowels. The case referred to was that of an old man who had hemorrhoids, which occasionally bled when he was in health. During the time specified, notes of near two thousand cases have been taken.

"*Hemorrhage into the cavity of the arachnoid*, which was not found in a single case of typhoid fever, had occurred before death in one-eighth of the cases of typhous fever.

"The amount of serosity found within the cranial cavity was decidedly greater after typhous than typhoid fever.

"*Pharynx*.—After typhoid fever, this organ was found ulcerated in one-third of the cases. After typhous fever, ulceration of the pharynx was not to be detected in a single case.

"*Larynx*.—Ulceration of the larynx was found in one of fifteen subjects dead from typhoid fever—in one of twenty-six from typhous fever.

"*Esophagus*.—After typhoid fever, ulcerated in one of fifteen cases in which it was examined. After typhous fever, the esophagus was free from ulceration in all the twenty-four cases in which it was examined.

"The epithelium separated from the esophagus spontaneously at an earlier period after death from the latter than the former disease.

"*Stomach*.—In none of the fifteen cases examined after death from typhoid fever was the mucous membrane of the stomach softened throughout its whole extent; in no case did the softening of the cardiac extremity approach perforation.

"In four of thirty-seven cases of typhous fever, the whole mucous membrane of the stomach was softened; and in four others, there was such extreme softening of the whole of the coats of the great *cul-de-sac* that they were perforated by the slightest violence.

"*Small intestines and mesenteric glands*.—The presence or absence of lesion of these organs was the ground on which the cases of typhoid and typhous fever here analyzed were divided from each other; consequently they were invariably diseased in the one and normal in the other.

"*Large intestines*.—After death from typhoid fever, the mucous membrane of the large intestines was found ulcerated in rather more than a third of twenty cases. In no instance after death from typhous fever.

"*Peritoneum*.—As peritonitis was in typhoid fever secondary to, and dependent on, the entero-mesenteric disease, it may here be excluded from consideration.

"*Spleen*.—This organ was enlarged in all the cases of typhoid fever—softened in one-third of the cases only. Before the age of 50, it was as large after typhous as typhoid fever; after that age, it was decidedly smaller in the former than in the latter affection. After the age of 50, it was as soft in typhous as in typhoid fever; before that age, it was frequently softened.

"*Gall-bladder*.—There was ulceration of the lining membrane of the gall-bladder in one of fourteen cases of typhoid

fever; in none of thirty-one cases of typhous fever. In the latter disease, the bile was much thicker and of a darker green color than in the former.*

“*Liver, pancreas, kidneys.*—These organs were more flabby in the cases of typhous than in those of typhoid fever.

“*Urinary-bladder.*—This viscus was ulcerated in one of the cases of typhoid fever—in none of the cases of typhous fever.

“*Pericardium.*—This cavity contained a small amount of yellowish, transparent serosity in all the cases of typhoid fever examined. The contained serosity was red, from transudation of a solution of hæmotosin, in five of thirty-one cases of typhous fever, in which the pericardium was examined before the termination of the fever.

“*Heart.*—The muscular tissue of this organ was much more frequently and decidedly flabby, and its lining membrane was much more frequently and deeply stained of a dark-red color, in the cases of typhous fever than in those of typhoid fever.

“*Lungs.*—Granular and non-granular lobular consolidation were very frequent in the subjects dead from typhoid fever; rare in those dead from typhous fever. The reverse was the fact with reference to consolidation from congestion of the most depending part of the lung.

“*Pleura.*—Recent lymph or turbid serosity was found in six of fifteen cases of typhoid fever—*i. e.*, between one-half and one-third, or in the proportion of forty per cent. The same lesions, but much less in amount, were found in two only of thirty-six cases of typhous fever—*i. e.*, one-eighteenth, or in the proportion of 5.5 per cent.

“The particulars here briefly recapitulated, appear to me to prove indisputably that the symptoms, course, duration, anatomico-pathological lesions, and the tendency to cadaveric changes, are different in typhoid fever to what they are in typhous fever.

“To account for the differences in symptoms which exist in continued fever, with or without entero-mesenteric disease, the following assertions have been put forward:

“1. That typhoid fever is merely typhous fever complicated with lesions of a particular organ; and, therefore, it is to be expected that certain symptoms referable to, and dependent on that lesion, will be present, and so far modify the symptoms of the disease. If the symptoms and signs referable to the intes-

*The condition of the bile, as found after death in these two diseases, is worthy of more careful investigation. The difference in appearance is, in a large majority of cases, well marked.

tinal disease as a cause—*i. e.*, the condition of the tongue, the diarrhea, increased resonance, and fullness of the abdomen, gurgling in the iliac fossa, pain and tenderness in the same region from the fluctuation of the contents of the bowel—were the only symptoms by which typhoid fever was separated from typhous fever, although the idea might cross the mind that they were two diseases, no sufficient ground for their separation would be present, unless the specific cause of the one was proved to be different from that of the other. But, putting aside the symptoms strictly referable to the abdominal lesion, the general symptoms of the two diseases, in the cases here analyzed, differed widely; such differences having no apparent connection with the local affection, but being probably, like it, dependent on some common cause acting on the whole system simultaneously.

“Thus the remarkable difference in the kind, not simply the amount, of the rash in the two diseases; and the tendency to local inflammations, to erysipelas, and to ulceration, observed in the cases of typhoid fever here analyzed, can not, with any show of reason, be considered to have been dependent on the disease of Peyer’s patches—*i. e.*, in the same way as the abdominal signs undoubtedly were; and it is to be carefully borne in mind that the external, the hygienic conditions of either group of cases were precisely the same in all respects. They occupied the same wards, partook of the same diet, slept on the same beds, under the same amount of clothing, and had the same physicians to attend them, and the same nurses to wait on them.

“Moreover, of the symptoms common to the two, the headache continued longer, and the delirium and somnolence came on, as we have seen, much later, in typhoid than in typhous fever; and the delirium, too, possessed a more active character. These differences, also, can not be explained by the presence of intestinal disease in the former, and its absence in the latter affection.

“The short comparative duration of the cases of typhous fever here considered, is another remarkable point of difference, totally inexplicable by the hypothesis that typhoid fever is typhous fever with intestinal ulceration. Had the cases eventually recovered, it might have been said that the intestinal lesion prolonged the disease in the cases of typhoid fever; but that all the fatal cases of fever, with a local lesion of so severe a nature as that recorded to have been present in the cases of typhoid fever, should have had a much longer course than all those other fatal cases of fever in which no organic change of structure could be detected after death, appears to me inexplicable, on the supposition that the former is simply the latter disease, with this serious lesion superadded. Let me repeat,

by this hypothesis we are asked to imagine that death is retarded in fever by extensive ulceration of the small intestines, and enlargement, softening, and even suppuration of the mesenteric glands. Surely, it behooves the supporters of such a statement to bring forward cogent proofs of the identity of the specific cause of the two affections ere they ask us to admit its truth.

“The same mode of reasoning appears to me equally conclusive, when we consider the comparatively early period of the disease at which the patients suffering from fever lost the ability to make muscular exertion. For, to suppose that the presence of abdominal complication in fever invariably prevented the extremely early supervention of debility is, *a priori*, still more absurd than to suppose such lesions to have retarded death. How, again, are we to explain, if we regard typhoid as typhous with abdominal complication, the differences observed in the ages of the patients; in their general manner; the muddy hue of the skin and uniform flush of the face, the injected conjunctivæ and contracted pupils, in typhous fever; and the comparatively clear complexion, the pink flush limited to the cheeks, the pale conjunctivæ, and the large pupils, in typhoid fever?

“In what way, also, are we to account for the differences observed in the physical breath-signs, on the supposition that the one is merely the other with abdominal complication?

“Death itself, moreover, adds new proof to the non-identity of the general affection in the two diseases. The comparatively rapid loss of muscular rigidity, the discoloration of the surface, the more flabby condition of the heart, liver, and kidneys, the extreme softening of the stomach, and the early separation of the epithelium, after typhous fever, are all cadaveric changes, by which death makes us cognizant of a condition of the system at large, which condition must have existed anterior to the cessation of life from that disease; and which condition could not have been present in the cases of typhoid fever, or death would have made it manifest.

“I need not here more than advert to the differences observed in the lesions which death simply enabled us to lay bare. The almost constantly-congested brain and membranes in typhous fever; the frequent presence of the signs of pre-existing serous inflammation in typhoid fever; the difference in the nature of pulmonary lesions in the two—are inexplicable on the supposition that the one disease is the same as the other, excepting so far as concerns the abdominal affection.

“Thus tried by facts—*i. e.*, by recorded symptoms and

lesions—the assertion that typhoid fever is merely typhous with abdominal complication, is completely refuted.

“2d. But another mode of explaining the differences which exist between the two diseases have been given—*i. e.*, that the differences observed depend on variations in the epidemic constitution. These cases afford a complete answer to this assertion. For a majority of the cases here analyzed of both diseases were observed during the same epidemic constitution. If the reader will refer to vol. xix, p. 668 Amer. Jour. Med. Sci., he will find that nineteen of the cases of typhous fever I have used were collected between May and November, 1848; and that thirteen of the cases of typhoid fever were collected during the same months of the same year. For such as prefer broad, general assertions to the details of a particular but more limited facts, I may remark, that during the three years’ attentive watching of nearly all the cases admitted to the London Fever Hospital, in which time there have been epidemics of relapsing fever, typhous fever, and cholera—and, consequently, according to those whose opinions I am here examining, as many changes in epidemic constitution—I have seen no alteration in the general or particular symptoms of either typhous or typhoid fevers, or the lesions observed after death from either—*i. e.*, from November, 1846, to November, 1849. The cases of typhoid fever—which disease is rarely absent for a fortnight from the wards of the hospital—preserved their symptoms unchanged, and presented the same lesions, whatever the epidemic constitution that prevailed; the same is true of typhous fever. Cases of the latter disease are also rarely absent from the wards of the same institution. It is there common to see patients occupying beds side by side, and presenting respectively the well-marked characters of either disease.

“But to return to the particular cases before analyzed. Allowing to epidemic constitution all the power of modifying disease claimed for it by certain writers, it must be granted that whatever influence this epidemic constitution exercised over the group of cases without intestinal lesion, it ought to have exercised over the group of cases with intestinal lesion, because the cases of the two groups were scattered indiscriminately over the space of two years only. If, I repeat, the two affections were really the same disease, then the same epidemic constitution ought to have impressed on both the same general features, implanted in both the same local lesions, and given to both the same tendency to cadaveric changes, and this allowing for all the modifying influence which the accidental presence of the abdominal lesion in the one and its absence from the other group might have occasioned. The analysis of every symptom, and every lesion, shows that the two affections were not thus assimilated by the prevalence of any particular epidemic constitution. But

if this epidemic constitution, by any stretch of the imagination, could be supposed to change from week to week, to cause the case attacked to-day to have typhous fever, the individual who takes the disease to-morrow to have typhoid fever, still, it could not account for the fact—as well established as any fact in medicine—that typhoid fever rarely, if ever, affects persons more than fifty years of age; while age exerts little influence in determining the occurrence of typhous fever.

“Thus, then, the assertion that typhoid fever is merely typhous fever modified by the prevailing epidemic constitution, is as irreconcilable with facts, as that the former disease is simply the latter with abdominal complication.

“To conclude:—In a former paper, I proposed to examine whether typhoid fever and typhous fever differed from each other in the same way as small-pox and scarlet fever differed from each other; and for the purpose of comparison, I laid down certain grounds, as those on which we founded our belief in the non-identity of the two last named diseases. Those grounds were:

“1. In the vast majority of cases, the general symptoms differ—*i. e.*, of small-pox and scarlet fever.

“[This holds equally true with respect to the general symptoms of typhoid and typhous fevers.

“2. The eruptions, the diagnostic characters, *if present*, are never identical—*i. e.*, in small-pox and scarlet fever.

“[The particulars detailed in the foregoing papers prove that this is true of the eruptions of typhoid and typhous fevers, as of those of small-pox and scarlet fever.]

“3. The anatomical character of small-pox is never seen in scarlet fever.

“[Just in the same way, the anatomical character of typhoid fever—*i. e.*, lesion of Peyer's patches and the mesenteric glands—is never seen in typhoid fever.]

“4. Both—*i. e.*, small-pox and scarlet fever—being contagious diseases, the one by no combination of individual peculiarities, atmospheric variations, epidemic constitutions, or hygienic conditions, can give rise to the other disease.

“[In this paper, I have not attempted to determine how far this holds true with respect to the diseases here treated; but I have considered it in a paper read before the Medico-Chirurgical Society of London, December, 1849), [Amer. Jour. Med. Sci., vol. xx., p. 384], the contents of which I may anticipate so far as to state that, to my mind, the origin of the two diseases from distinct specific causes is as clearly proved as that scarlet fever and small-pox arise from distinct specific causes.]

“5. The epidemic constitution favorable to the origin, spread, or peculiarity in form or severity of either—*i. e.*, small-pox and

scarlet fever—has no influence over the other, excepting that which it exerts over disease in general.

“[The facts detailed in this paper prove that this holds as true of typhoid and typhous fevers as of small-pox and scarlet fever.

“If, then, the above are the grounds—and, after mature deliberation, I am able to assign no others—for the separation of small-pox from scarlet fever, I think it is indisputably proved that typhoid fever and typhous fever are equally distinct diseases; not mere varieties of each other, but specifically distinct—specific distinction being shown in typhoid and typhous fevers, as in small-pox and scarlet fever, by the difference of their symptoms, course, duration, lesion, and *cause*.

“Before closing this paper, I ought to observe that, with respect to some secondary points—*e. g.*, the chronological relation between the laryngeal and pharyngeal affections—it may be considered that I have drawn general conclusions from a too limited number of facts. But a few facts, impartially observed, minutely recorded, and carefully analyzed, are, I believe, more likely to give correct results than a multitude of general observations; and moreover, I believe most men would be astonished, if they had in numbers all the cases of any given disease they had ever seen, yet concerning which they have generalized. The method I have adopted, however prolix it may be, however difficult to conform to, however tedious the details into which it leads, has this advantage, that, if the observer be honest and capable of noting what is before him, thinking men may judge of the value of his facts, the force of his reasoning, and the correctness of his conclusion; whereas, general observations, while they are incapable of proving anything, are exposed to all the fallacies of definite statements, because the one, like the other, rests ultimately on the accuracy of the facts observed. If the observations, on which any reasoning is founded, be erroneous, no cloaking of those observations, in general terms, can render the conclusions correct. It has been objected to definite numerical statements, that they mislead the reader by an *appearance* of accuracy, in cases where there has been great inaccuracy in observation. This objection appears to me to lie against the condition of the reader's mind, and not against the method. For if the reader fails to examine, 1st, the trustworthiness of the author, and, 2dly, the legitimacy of his conclusions, the fault is, obviously, mentally his own, and in no way to be ascribed to the method. Because chemists have, by the imperfection of their analysis, arrived at incorrect conclusions as to the ultimate constitution of various organic bodies, we surely would not have them henceforth confine themselves to the general impressions pro

duced on their minds by a series of experiments or observations. The more complicated the problem to be solved, the more careful ought we to be that *every* step in its solution is made correctly. How complex questions, such as arise in medicine, are to be determined mentally—*i. e.*, without the aid of figures—by ordinary men, I am a loss to conceive. Yet physicians think to solve, by mental reveries, problems in comparison with which the most difficult that the most renowned mental calculators ever answered, were child's play; and not only do they think to solve these problems, but to carry in their minds for years the complicated materials by which they are to be solved.

“Who can tell what general statements are worth, without knowing on what evidence they rest? One man's many, is another's few. Last month (October), I saw thirty cases of fever. To me, these were few; to men with smaller opportunities of observing that disease, they would have been many. One man's frequent, is another's seldom.”

Speaking upon the use of stimulants in the treatment of continued fever, Dr. TWEEDIE, Physician to the London Fever Hospital, etc., in his recent Lumleian lectures before the Royal College of Physicians, says:

“It is always necessary to watch the effects of the first few doses of wine, and if the pulse abates in frequency, becomes soft and fuller, the tongue moist, and the heat of the skin not increased; and, when there has been delirium, if the patient becomes more calm, and has intervals of sleep, we may feel sure that the wine is doing good. On the other hand, if the pulse increases in frequency and strength, the skin becomes hotter, and the patient restless, flushed, and excited, with throbbing of the temporal and carotid arteries, we may consider either that wine is not suited to the case, or has been given too early, and should, therefore, be withdrawn. But, as a general rule, it is perhaps better to give wine a little too early than a little too late, since if it appears to disagree, it is easy to suspend its use; but it may be very difficult to restore the vital powers if they have been allowed to remain too long unsupported.

“Nor should the wine or brandy be discontinued until convalescence is fairly established; but as the symptoms for which the stimulants have been prescribed disappear, the quantity should be gradually abridged by giving smaller portions and at more distant intervals.

“In regard to the amount of wine and alcoholic stimulants that may be administered in typhus, no precise rules can be laid down, as the ever-varying circumstances presented by individual cases can alone determine this. It is prudent to begin with half an ounce or an ounce, and to repeat this amount at

longer or shorter intervals, according to the effect produced. From six to twelve ounces may be considered to be an average daily allowance, but sometimes it is necessary to give two or three pints, or even more, in twenty-four hours, and it is surprising to observe, without the slightest intoxicating effect, even when the patient has been previously unaccustomed to stimulants. Indeed, in low fevers, the exhausted state of the nervous system appears to be antidote to the effects of stimulants—in short, to create a tolerance of wine and diffusible stimulants.

“The wine should always be conjoined with nourishment, in order to assist its due assimilation, though in many cases the digestive powers are so feeble that they are unable to elaborate even the lightest articles of food, and, therefore, the wine or brandy may be given simply diluted with water.

“I have just alluded to the daily quantity of wine that it may be necessary to prescribe in typhus, and stated that no precise rules can be laid down, as the circumstances of each case must determine it. You are doubtless aware that there is a great tendency in the present day to revive the Brownian system, which flourished for a time in the latter part of the last century, in all acute diseases, including fevers, without regard to individual peculiarities. The doctrine inculcated by some teachers with respect to inflammation is, that this process being a deranged nutrition, involving supply and waste, and the waste being considerable while the inflammatory process lasts, there must be a compensating supply; that as the supplies for the formation of the abnormal products of pus and lymph must be drawn from the blood, or from the tissues, or from both, the vital powers become exhausted, in proportion to the organic disintegration that takes place. Hence it is concluded, that the more the inflammatory process draws upon the blood, the greater will be the exhaustion of vital force, and the consequent effect upon the whole frame.

“Upon this physiological theory of the phenomena of inflammation, is based the overthrow of established therapeutic principles, on which the treatment has been for ages conducted. But surely even the abettors of this theoretical view must admit that the object of treatment is to anticipate or prevent those so-called destructive processes: in other words, to promote resolution by all available means. Is this to be accomplished by extravagant doses of wine and brandy, regardless of the every-varying condition of the sufferer or period of the disease?

“Similar reasoning is adduced in regard to the phenomena of fevers, whatever be their type or special circumstances. It is against the indiscriminate employment of stimulants in fever that we protest, being convinced that their proper administra-

tion requires as much consideration as is generally bestowed on other measures employed as curative agents.

"The enormous quantities of wine and brandy recommended in even the early stage of fevers, whatever be the form, the individual circumstances, or whether there be local affections present, have often surprised me, and inclined me to doubt the accuracy of the statements. I have certainly seen intercurrent inflammations materially aggravated by the injudicious stimulation adopted, and on more than one occasion all the ordinary characters of acute delirium tremens supervene when the unlimited administration of brandy had been left to the discretion of a nurse, who fancied that she was only obeying instructions when she poured down dose after dose of pure brandy. There is surely no practical philosophy in such indiscriminate abuse of a really valuable remedy when given on rational principles; and I deem it the duty of every physician who is convinced of the dangerous tendency of the Brownian doctrine applied indiscriminately in the treatment of diseases, acute as well as chronic, to express his opinion boldly and decidedly, that the young and inexperienced practitioner may be warned of the dangerous consequences of this recently revived doctrine. * * *

"Let me also allude to the importance of giving the wine at stated intervals, and only when the excitement is moderate. It is especially necessary to give it during the night, when there is often great exhaustion. A dose of wine judiciously given at this diurnal period is often followed by calm, refreshing sleep; and hence the incalculable advantage of an interested, experienced nurse, on whom so much responsibility—indeed the life of the patient—often rests."

On the question of change of type in fever, Dr. TWEEDIE says: "If we examine closely this theory as applicable to the acute diseases of the last thirty years—and this can only be undertaken by those who have witnessed and studied their type during the period referred to—and weigh dispassionately the evidence adduced, more especially by nature herself, I apprehend that the true explanation of the difference of treatment will be found to consist in the more cautious or restricted notions now entertained as to the necessity for the heroic remedies formerly so freely, and I may say indiscriminately, adopted. Even those who are in favor of this doctrine, can not assert that the pathological phenomena of acute diseases have undergone a change, for the symptoms, general and local, and all the essential morbid processes of the entire class of pyrexial diseases, have not undergone the slightest alteration; and if the evidence as to the depressed or asthenic condition of the vital powers be scrutinized, I have a strong impression that the conclusions adopted, more especially in reference to fevers as a class, are not warranted by facts. And how important is it to form a

correct judgment of this doctrine, since it determines the line of treatment to be pursued, and may even involve the safety of many valuable lives!

“The subject has not escaped my attention, and I have come to the conclusion, that though certain cyclical differences in acute diseases, of longer or shorter duration, may have been occasionally detected, the notion of change of type, as regards the various forms of fever, has been greatly exaggerated.

“Let me state the grounds upon which I have come to this conclusion, which I admit is at variance with the ideas of many physicians whose knowledge and judgment entitle them to great consideration.

“We find that Sydenham, who is considered to be the author of this change-of-type theory, cautioned the medical men of his day against too hastily determining the treatment of a new epidemic—until, in short, the practitioner and the disease were better acquainted—on the reasonable ground, that epidemics assumed at one time a more acute or phlogistic, at another a less acute, or asthenic character; but we do not find that he had observed a change in one direction only, and for so lengthened a period as considerably more than a quarter of a century. If the records of epidemics of other varieties of fever be examined—the eruptive, for example—it will be evident that, during the same period (in small-pox, measles, and scarlet fever), every variety or modification of type has been observed, the type being sometimes acute, sometimes more or less asthenic, and requiring, consequently, variation in treatment.

“I apprehend that the true explanation will be found in the fact that, until very recently, little or no attention has been paid to the ever-varying differences in form which fever assumes—at one time typhus, at another enteric or typhoid, or it may be relapsing fever, constituting the features of the prevalent fever, though it should be kept in view that, whatever be the character or type of an epidemic, individual differences arise, according to the peculiar circumstances in which a single individual, or a number of persons, or a community, may be accidentally placed. The question of the identity or non-identity of the several forms of continued fevers thus becomes of the greatest importance in relation to the change-of-type theory. For example, the great argument adduced by those who support the doctrine is, the decided results in the Edinburgh epidemic of 1817–20—and which I had the opportunity of witnessing—of the large indiscriminate bleedings in diminishing the mortality. This argument, however, loses much of its intended effect when it is considered that by much the larger number of cases consisted of relapsing fever—a form the mortality from which has already been shown to be exceedingly small under any kind of treatment; and that the death-rate

has been even less when no blood was abstracted at all. As in other epidemics, the mildness or severity of the fever has varied at different times. We are told, somewhat exultingly, that under the unnecessarily profuse phlebotomy practiced in 1817-20, the mortality did not exceed 1 in 22 at any period of the disease, and was reduced so low as 1 in 30 as the epidemic spread; but in the argument it has been overlooked that the mortality of this fever is liable to much variation. For example, in the epidemic of 1843, the history of which has been given by Dr. Cormack, the deaths were 1 in 16; of the cases recorded by Dr. Wardell (1843-4), it was 1 to 20; and of 203 cases treated in the Edinburgh Infirmary in 1848-9, there were only 8 deaths; and if we extend our inquiries to other places, we find that of 7804 cases of relapsing fever admitted into the Glasgow Infirmary, between the years 1843 and 1853, the deaths were 405, or about 5 per cent.; and in the London Fever Hospital, of 441 cases admitted during ten years (1848 to 1857), 11 died, or in the ratio of about 1 to 40.

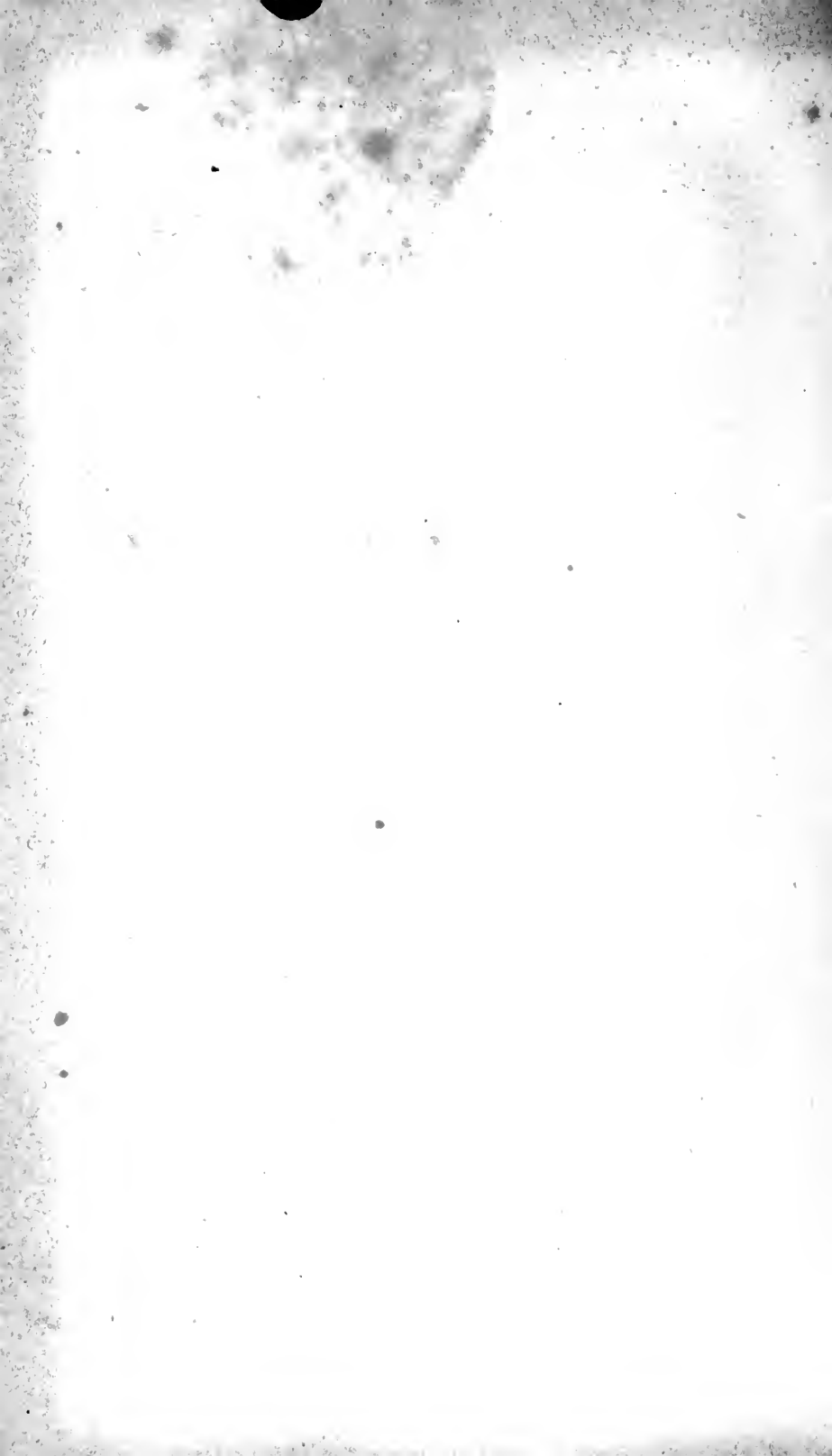
"This variation in the mortality could not be ascribed to the remedies employed; for Dr. Cormack states, that having been urged by medical friends to test the effects of blood-letting, he instituted trials of this remedy, but candidly admitted that, though the symptoms were sometimes evidently relieved, the beneficial changes were often not effects but sequences of the bleeding, as was satisfactorily proved by the very same changes frequently occurring as suddenly and unequivocally in patients in the same wards, and affected in the same way, *who were subjected to no treatment whatever*. And in regard to the treatment instituted at the London Fever Hospital, when the mortality of relapsing fever did not exceed one in forty, with scarcely an exception, no blood was abstracted at any period of the disease.

"It is clear, therefore, that the change-of-type theory can not rest on comparison of the treatment by indiscriminate phlebotomy formerly practiced, when all acute diseases, including fevers, were supposed to be under the dominion of the lancet.

"But though the grounds on which the question has been argued are, in my opinion, erroneous, one good result has followed in the death-blow which the practice of indiscriminate phlebotomy, formerly adopted in all acute maladies, has received; for, too often, little or no regard was paid to individual peculiarities, or even to the stage of the disease for which the bleeding was employed. The inquiry was simply as to the existence of fever or of inflammation; and, the question once settled, the lancet was unsheathed, and much blood unnecessarily shed, and from the effects of which the patient did not recover, perhaps, for months. But, on the other hand, there is great hazard of many important diseases being allowed to

gain the ascendancy from the indecision that has resulted from the complete alteration of therapeutic principles which the discussion of this question has brought about. * * *

"It is consoling to observe that, in the present day, there is a more just appreciation of the powers of curative agents, as well as of the principles on which they should be applied, not only in acute but in chronic diseases. This is the consequence of studying the effects of disease on individual structures, and of the efficacy or inefficacy of remedies to subdue the changes that take place when certain morbid actions have become established. We are now, or ought to be, satisfied, that the most scientific, as well as the most successful course, in many acute diseases, after a certain period, is, not to interfere too much, if at all, with the operations of nature in her efforts to repair the injury the parts or organs have sustained by disease."



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